THE JOURNAL OF PARAPSYCHOLOGY

Volume 15 DECEMBER, 1951 Number 4

Editorial PAGE

Proposals for a New Symposium and for a Parapsychology Forum 227

The Present Outlook on the Question of Psi in Animals 230
J. B. RHINE

Introversion-Extraversion Ratings in Relation to Scores in ESP Tests 252
BETTY M. HUMPHREY

Minor Articles

A Comparison of Two Subjects in PK 263
PAUL AND CHRISTIANE VASSE

An Experimental Study of ESP Capacity in Mental Patients 271
K. E. BATES AND MARIETTA NEWTON

Notes and Comments

Reflections on Parapsychology, Psychoanalysis, and Atomic Physics 278
PASCUAL JORDAN

Book Reviews

The Clairvoyant Theory of Perception by M. M. Moncrieff .. 282
C. J. DUCASSE

Glossary 288

A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

The Journal *of* Parapsychology

EDITORIAL STAFF

J. B. RHINE, J. G. PRATT, and BETTY M. HUMPHREY, *Editors*
J. A. GREENWOOD and T. N. E. GREVILLE, *Statistical Editors*
DOROTHY H. POPE, *Managing Editor*

ADVISORY EDITORIAL BOARD

HORNELL HART, Duke University
H. H. PRICE, University of Oxford
GARDNER MURPHY, College of the City of New York
ROBERT H. THOULESS, University of Cambridge

This JOURNAL is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $6.00 a year; single current numbers $1.50. The rate for back volumes is $7.00; for single numbers $1.75. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

DUKE UNIVERSITY PRESS

The Journal *of*
Parapsychology

Volume 15 DECEMBER, 1951 Number 4

EDITORIAL

PROPOSALS FOR A NEW SYMPOSIUM AND FOR A PARAPSYCHOLOGY FORUM

MUCH HAS BEEN said in this section of the JOURNAL about the status of parapsychology—about trends in the research, about the bearing of its findings on related problems, and about the outlook for parapsychology in the future. The general response of our readers to the concepts and evaluations which we have suggested has been gratifying. This reader reaction, however, has not included anything like a healthy amount of disagreement with the interpretations and judgments registered in these editorials, and on such matters there is bound to be a large amount of difference in viewpoint. It seems likely that this silence may be due to the lack of an adequate and convenient medium for the free exchange of opinion and for the expression of varying interests. The value of such free exchange is apparent enough to justify special effort on our part to be sure that it is maintained and that critical opinion is never allowed to pass unnoticed. Accordingly we are making two suggestions here for consideration by our readers, with an earnest invitation for comment. We hope in this way to share some of the responsibility for deciding editorial policy.

The first suggestion is that we hold another symposium in the JOURNAL such as the one published in March, 1948, which was entitled "A Program for the Next Ten Years of Research in Parapsychology." The earlier symposium proved to be of great help in

instituting much needed thinking about long-term research aims for the field. It was a most successful editorial undertaking.

It is proposed that the subject for the contemplated symposium be the appraisal of the present state of progress and of current developments in parapsychology. From ten to twenty persons who have been actively engaged in doing something about parapsychology during the past five years would be asked to contribute a short discussion of this topic. For such a symposium to serve its purpose well the commentators should represent a wide range of approaches to parapsychology, and need not be contributors to psi research. The invitation should be extended to representatives of fields adjacent to parapsychology in order to bring to the discussion the advantage of differences in viewpoint. At the same time, there need be no attempt to obtain comment from every qualified and interested judge of parapsychological progress; there would not be space available for a coverage of that scope.

The contributors should of course be asked to write what they think about present trends and emphases in the research, what is most needed in the way of constructive changes, and how these improvements could best be brought about. They should be requested to give frank evaluative criticism and free constructive proposals of specific character.

Let us warn ourselves in advance, however, that no one should expect any large amount of agreement to emerge from such a discussion. The good which is more likely to result will come from the broadly corrective educational effects upon those who survey these various views with the symposium authors. Everyone may expect to have a widened perspective and to be better for having his own conceptions challenged.

The second suggestion is that parapsychology needs a public forum, and that a small section of each issue of the JOURNAL OF PARAPSYCHOLOGY should be set aside for this purpose. The Parapsychology Forum as tentatively conceived would be primarily educational. Parapsychology has little as yet to offer its students in the way of an educational program and the Forum should provide, first of all, a means of answering many questions handled at present only in individual correspondence and should result in wider dissemination of knowledge with a greatly reduced effort.

Also, for a field that has so many debatable questions there are surprisingly few published debates in parapsychology. As a second type of function, therefore, the Forum could present appropriate issues that would not ordinarily be brought up either as subjects for research papers or as interpretative articles of the conventional type. Various viewpoints would be represented. Debatable topics and points of confusion could thus easily be clarified as far as this is possible at present.

As a third service to readers there should be provided some place for the expression of claims, hypotheses, and accounts of observations on a more tolerant level of scientific responsibility than we can in safety allow for the JOURNAL in general. There should, of course, be an appropriate statement at the heading of this more liberal section of the JOURNAL advising the reader of the degree of responsibility assumed.

The Forum would be frankly a trial project, for no one can be certain how valuable and how interesting such an exchange of ideas will turn out to be. It is conceivable that in time the Forum might outgrow the space it could be given in the JOURNAL. It is also conceivable that it might not be handled in such a way as to draw sufficient interest from the reader clientele to maintain it. Still another possibility is that it might metamorphose into something even better than our present conception of it under the guidance of experience and of helpful suggestions that may be received.

Above all, the greater informality and the closer relationships should result in benefits not only to editors, authors, and research workers, but to the larger group, the readers, as well, at least so long as no appreciable amount of formal instruction in parapsychology is available.

The reaction to these two suggestions will not be unbiased unless it includes an equal readiness to approve and disapprove and a freedom to offer any countersuggestions that may occur as to better ways of achieving the purposes expressed. Should these new proposals find approval we should welcome topics, questions, and other pertinent suggestions as a part of the response to this announcement. The concept of this plan will never be so fluid and modifiable as at its inception.

Very well, then, let the reader write—pro and con!

THE PRESENT OUTLOOK ON THE QUESTION OF PSI IN ANIMALS

By J. B. Rhine

It was indicated in the Journal more than a year ago that something of an excursion was being undertaken into the field of biology in search of psi in animals. This move has met with universal approval so far as may be judged by the indications given; no comparable departure in parapsychology in the past was received with more general favor, and for that matter, probably no other development was more manifestly needed.

Parapsychologists themselves recognize the need for a wider foundation for the growing body of knowledge being accumulated. To the concern over the place of psi in the personality of man which has occupied attention now for some years, there is being added definite interest in the related physiology and especially the neurophysiology of psi. The fact that psi is extrasensory and (in PK) extramotor naturally delayed an inquiry into the question of its neurological foundation, but it is a question that cannot be put off indefinitely.

The time has come also when the question of what we know about the hereditary basis of psi capacity is more repeatedly asked. During the period when parapsychology was struggling for its existence such a problem was somewhat academic, but this is no longer true. It must be supposed, of course, that any natural ability of the individual owes something to hereditary endowment, and the time has come to look into the question of what that endowment is in the case of psi. The same is true of the problems of where psi capacities first entered the evolutionary picture, whether early or late, whether with man or with his remote or recent ancestry. To know that would tell us a great deal about that capacity. And, finally, what is the relationship of psi to the directive functions of the organism, to life itself?

Even those who have a far-seeing interest in the historic parapsychological question of whether there is anything in man that survives bodily death can see that we should ask first whether there is

anything in human personality beyond the physical operations of the organism. It would be a definite step in that direction if it should turn out that psi is a general property of living organisms, since this would identify an extraphysical function with life itself. Since psi offers the one scientific challenge thus far presented to the physicalistic theory of the organism, the substantiation of this hypothesis would at least open the way for the possibility of an extrasomatic element in the personality that might survive organic death. It takes no little resolution to keep such a question in perspective and in the open in our present intellectual climate. Some will scorn even to ask it, and others still will not easily yield its formulation to terms as definite as these. If, however, the question is to be kept in the focus of scientific attention, I think it is in this biological context that it can best be understood.

It is interesting to note that this question of psi in animals actually originated among zoologists. Students of migration have more than once suggested that telepathy or some "sixth sense" may be involved in the more exceptional direction-finding shown especially by birds in long-distance flights over open water or unfamiliar land masses. Also, we have quoted in a recent number of the JOURNAL the late Professor F. B. Sumner's suggestion that extrasensory perception may be necessary to explain the migratory return of the salmon. Other leading zoologists, such as Professor A. C. Hardy and Dr. Julian Huxley, have in recent years mentioned psi capacities as matters that biologists will have to take into account.

This tolerant interest, however, is not confined to these few. The many contacts made in recent months with other zoologists who are actively at work on problems involving direction-finding in animals has revealed that there is a general readiness to entertain new hypotheses to account for the more exceptional powers of orientation shown by many species of animals when displaced beyond their natural territory. There is apparent, on the surface at least, much less of the fear of the unfamiliar that has characterized the reaction to parapsychology by some of its neighboring sciences in the past. This difference may be due in great part to the fact that for a century biology has been trying in vain to solve the problem of how animals find their way in their more exceptional migration and homing, and is more ready in consequence to consider new hypotheses.

Zoologists have shown no lack of readiness to discuss the ESP hypothesis in connection with the unexplained behavior of animals in finding their way in unfamiliar territory. Once it is made clear to them that the approach in parapsychology is objective, empirical, and cautious, there is no apparent fear of where the experimental facts may lead. No hypotheses will be ruled out in advance and there will be no overruling bias that will distort interpretations of results. The question of whether these direction-finding performances of animals involve ESP or whether they involve something unknown in the range of sensory perception should be answered by the methods of natural science appropriate to the problem.

* * * * *

On this question of psi in animals the most challenging material, by all odds, is the evidence of unexplained powers of direction-finding. That there are certain migratory movements involving a wide range of species that have never had an acceptable explanation is in general frankly recognized. In long-distance homing experiments, too, many species of animals manifest a puzzling ability to find their way in territory known to be unfamiliar to them. There is in such cases the dual problem, first, of how the animal knows in which direction home lies and, second, that of how it is able to maintain such a direction consistently enough to reach home. The home location is in some way apprehended, and the right orientation toward it is maintained over the period required.

It is only in certain of the migratory phenomena, however, and only in a part of homing behavior, that there is any need to seek and consider new hypotheses. There can be little doubt that in a great deal of migration and in short-distance homing, the known perceptual equipment of the animal affords an ample basis of explanation. Moreover, an inquiry that attempts to deal with the explanation of direction-finding and home-finding over longer distances must take into account the fact that we cannot suppose that the full range of sensory capacities has been exhaustively investigated as yet. The comparative recency of discoveries of the bat's capacity to respond to supersonic vibrations of the air, of the capacity of bees and ants to utilize polarized light from the sun in maintaining direction, and of certain birds to orient themselves by the angle of the sun's position, should help to keep us reminded that there may

be many such extensions yet to be made to the scope of the animal sensorium.

Other factors in perceptual judgment, too, need to be kept in perspective for a possible place in the ultimate explanation of the observed phenomena; for example, the established fact that certain birds can be taught to respond reliably to number; that bees, at least, can communicate, with almost incredible accuracy, the precise angle to be taken in search for food; and the growing impression that certain animals have an appreciation of time (though it may not be entirely clear as yet whether the response is made to an associated cue or sign instead of on the basis of time judgment itself). The hypothesis of bird navigation by means of the earth's magnetic field and coriolis force has apparently not been gaining ground, but there is the suggestion that one of the electric fishes may use a type of radar.

On the whole, there is an interesting array of possible sensory extensions both in kind and degree which the investigator looking for ESP in animals must carefully consider. Since the mode of operation underlying the working of ESP is still less understood than it is in the case of any of the sensory phenomena, we shall feel that we are on firmer ground if we accept a sensory explanation as the more plausible wherever it is a reasonable possibility. As always, the area for psi investigation is that which unquestionably lies beyond the realm of sensory function.

Our first need in dealing with the question of psi in animals is to make sure that we have a good enough case to justify a thoroughgoing investigation. The type of phenomenon most often cited by the zoologist who is ready to consider some unknown mode of perception might be briefly reviewed. For some it is the migration of the European eel from its spawning area in the deep water east of Bermuda to the rivers of western Europe and north Africa, and its return after several years to the breeding location near Bermuda. Others are most impressed by the return of the salmon after its long period of wandering about the ocean and its compulsive struggle up the same stream it descended in its emigration. The ornithologist is more likely to cite the flight of the golden plover from its Alaskan summer home to the Hawaiian or other Pacific islands, flying day and night over at least 2,000 miles of unbroken water; or the At-

lantic species of plover which takes off for its oceanic flight from the tip of Nova Scotia and flies over water to the Lesser Antilles and the northeastern shoulder of South America, a still longer flight over water than its Pacific cousin makes. Some are most puzzled by the ability of certain species of birds like the great shearwater to find the tiny mid-Atlantic island group, Tristan da Cunha, where the species assembles each year for breeding on return from the summer migration to the north in the neighborhood of Greenland. The fact that in many species the migratory flight goes on both day and night makes the problem still more complicated for a sensory explanation. Yet another angle of interest is introduced by the fact that in certain species of birds the young and old migrate at different times, making it reasonably probable that whatever the basis of orientation is, in the first year at least it owes nothing to memory or to familiarity with the migratory flyway.

These are admittedly puzzling phenomena of animal behavior, and it is perhaps little wonder that there has been much talk of an unknown "sense of direction," a "faculty of orientation," a "sense of location," a "sixth sense," and even telepathy. There are some biologists, however, who hold conservatively to a view that some combination of the known sensory equipment, random exploration, and a certain amount of luck (favoring air currents and the like), must guide the species through its difficult wanderings, especially since the migration is not always without severe losses. Much depends in these interpretations, of course, upon whether one wishes to focus mainly upon the migration that is easiest to explain or the more challenging and exceptional performances. In our parapsychological excursioning into this field we are frankly looking for the most exceptional.

It appears logical enough, we think, to consider the hypothesis of ESP as a possible explanation of this migratory behavior of the more extreme type. Since ESP is an established human capacity (though how general may be its distribution it is too early to say), it is not unreasonable to suspect its presence in other species. If it had an early evolutionary origin it might very well be much more potent in lower animals than it is in man. Moreover, since ESP already has the status of fact in human behavior, it has a definite advantage over the supposition of such abilities as direction-sense,

sense-of-location, orientation-faculty, magnetic-sense, and the like, which have yet to be established experimentally. It is true that ESP runs counter to the philosophical bias against nonmechanistic hypotheses, and to a certain extent this philosophical attitude will affect the value placed on evidence; but we think there is already enough of a case for ESP in one species to offset to a great extent this deterrent philosophical resistance, especially when the matter is brought to clear-cut empirical test. Then either the philosophical bias or the ESP hypothesis will have to give way, and it is to that point the issue must be directed.

* * * * *

Mere field observations of migration, however, only raise the question of what guides the animal on its long passage. Other steps must be taken to answer the question. A great deal of field work has been done with migratory birds, capturing them, banding them, and transporting them out of their customary flyways into territory that was unfamiliar (Rüppell, Drost, Schüz, McIlhenny, Wojtusiak, Wodzicki, etc.). In general a large enough number of these birds have been recovered in the old flyway to impress the observers with the view that something more than just accidental return was involved. Some of the European observers noted that in later migration young birds so removed merely maintained a direction parallel to the old flyway but did not return to the old one. Only the older birds were found among the recoveries. The general view is that the greater familiarity of the older birds accounted for the difference.

A large part of the challenge of animal behavior to existing hypotheses comes from homing experiments with birds without the complication of migration. Many of these experiments were carried out with wild birds (by Rüppell, Griffin, Goethe, Lack and Lockley, Schifferli, Manwell, Schüz, Watson and Lashley, Wojtusiak, Wodzicki, and others). Other experiments in homing have been conducted with the carrier pigeon (by Riviere, O. and K. Heinroth, Kramer and St. Paul, Yeagley, Matthews, Gundlach, Hitchcock and others). In the experiments with the wild species the birds were trapped, usually at nesting time, and then after being banded and, perhaps, marked with paint for easy identification, were transported to a distance from the home site and released. The distances and directions were varied, usually so as to extend well outside of

the familiar territory and migratory routes of the species. The percentage of birds that returned and the speed of return were recorded when the birds were recovered and identified at the nest again. A number of wild birds such as gulls, shearwaters, storks, swallows, starlings, and others were found to be good homers without training, and in a number of the experiments it was abundantly clear that the birds were homing in territory entirely unfamiliar to them (as, for example, when marine birds were taken great distances inland). Recovery percentages were sufficient to require some other explanation than chance. The comparative success of long distance recoveries and the percentages of return, along with the comparatively greater speed of return from the longer distances, weighed against any sort of hypothesis of spiral exploration or random search on the part of the homing birds. They appeared to be taking a direct route homeward in enough instances to make explanation difficult on this basis.

Probably no one would want to conclude that these homing experiments are entirely conclusive and adequate. Taken together, however, they raise the question of the adequacy of known senses to explain direction-finding more pointedly than the migration studies mentioned, and they offer evidence that is more favorable to the ESP hypothesis. For one thing, most of these comparisons of distance in the homing experiments seem to indicate that degree of distance is not a factor of importance. In spite of the fact that the bird is exposed to enemies and other hazards more or less in proportion to the distance from home as well as to inconveniences and lack of adaptation to the food sources in strange country, there is not the amount of falling-off of recovery percentages with distance that would be expected if only random search and the known sensory equipment of the animal were involved. Even assuming a very liberal allowance for chance returns, which cannot be precisely estimated, the suggestion is pretty strong that some new explanation is called for, some extension of perceptual powers, such as ESP. At this stage of the inquiry, then, it is plain that the question must be kept open and additional, more definitive, experiments conducted.

When we turn to the investigations carried out with homing pigeons we find much the same situation, with perhaps a little sharper focusing of the experiments. The work of Kramer and

St. Paul show that the angle of the sun can play an important orienting role in the homing of the pigeon. This is supported by Kramer's experiments with wild birds in his special control cage which have shown that birds do reliably orient themselves in accordance with the sun angle. The work of Matthews, however, appears to be a step beyond this type of sun-angle explanation. While in his first work, done with one breed of pigeons, Matthews confirmed the other pigeon studies mentioned, he found that with another strain of stock the birds could not be thrown off by moving them to points of release at directions from the loft in which they had had no training or experience hitherto. It would seem that to explain this behavior not only a careful measurement of sun angle was required, but also a correction for the time of day to give the absolute position of the bird at the time of release. Since most of Matthews' pigeons turned the right way in general in the second experiment, no matter in which direction they were taken from the loft, some explanation other than simple sun angle is required. Extrasensory perception is such an explanation, but whether it is *the* explanation it is too early as yet to say. But at this stage it is enough to find pigeon experiments that come so close to being a test for psi. Recent unpublished work by Kramer confirms Matthews' findings that pigeons can home over long distances in a direction to which they have not been trained.

* * * * *

It is a good question, however, whether we may not find in the homing of mammals a still better target for our study of psi. Reports of homing in domestic or domesticated mammals have probably always been familiar, and they are to be found in some of the earlier textbooks in zoology. In our own day the most familiar of these cases concern dogs and cats.

In experimental homing, by far the greatest amount of research done with mammals has been devoted to mice. Both in Europe and in America and both in field and in laboratory experiments, studies have been made on mice homing that bring these rodents to the fore as another animal suitable for the investigation of psi. In Germany Schmid and Schleidt, and in this country the Muries, Burt, Hamilton, Stickel, and others have used mice in homing tests conducted in the field. The work as a whole strongly suggests that the mice

home from distances well beyond familiar territory and do so under conditions that make application of known sensory equipment apparently inadequate to explain the direction-finding. Here again, however, we must say that more work with further control is needed for a definite conclusion. But the direction is already indicated for the design of more conclusive experiments.

In the laboratory studies of direction-finding in mice by Neuhaus and with mice and rats by Krüger and Vogelberg, we see the advantages of laboratory controls even though they are imposed at some risk of interference with the capacities to be tested. The results are not so striking percentagewise as the field observations, but the interpretation of them may be easier. These experiments consist essentially in the attempt to train mice or rats (released within an enclosed multiple choice apparatus) to take a certain compass direction, say to the east, and to do so without any known sensory cue to identify the direction for them. The animal is placed in the apparatus without any guiding cue, and from trial to trial the situation is altered, so that if he consistently takes the right direction it supposedly means that he has a sense of direction or else some capacity which would encompass the ability to find the right direction.

The motivation of the animal is dual. If he goes in the right direction (east) he will find food awaiting him, and if he takes one of the several wrong directions, he will get shocked and will not be able to escape. The investigators found evidence that under such conditions certain animals (not all) could find their way out of the apparatus in a particular direction without error a statistically significant percentage of the time but by no means all of it. Whether or not the experimental controls are as yet completely adequate is a debatable point. Probably no one will dispute that a good question has been raised concerning behavior of mice and rats, that the value of these animals in direction-finding experiments has been demonstrated, and that there are strong suggestions of something beyond the known senses operating in their behavior.

There have not as yet been many systematic tests of homing with larger mammals.[1] Herrick's release experiment with homing

[1] Grzimek tried horses in release tests for homing ability, but none were successful. Gundlach tried cats and pigeons in a direction-finding test somewhat like the Neuhaus method. (The multiple-choice response could take one of three

in cats and Schmid's with dogs stand very much alone, though both found successful homing that at least justified further research. We see, however, no need whatsoever to confine our discussion to what we may dignify with the somewhat elastic term "experiment." Parapsychology owes much to the use in proper context of non-experimental, even anecdotal, material. It is just a matter of keeping the use of the material within the limits to which it is suited.

The case reports of homing dogs and cats have not been given much consideration in science, and we must depend for our collection of them upon popular records such as the daily press, magazine articles, and the books on cats and dogs. Once an extensive survey of this material is made, however, and large numbers of cases come under classification, certain things stand out that give one pause in his first reaction to brush the whole matter aside as not resting upon the foundation of scientific observation. During a study of the first dozen or so cases encountered, the natural apprehension about unreliability of the testimony, possible errors in identification of the animal, or even the conceivable alternative that some kind of hoax was perpetrated all haunt the judgment of the investigator. But as score upon score of cases are collected from different countries and different sources of information and the impressions are compared with those of observers of nearly a century ago, the impression is quickly lost that we are dealing with worthless hearsay material. Rather, one is forced to face the fact that here is animal behavior so frequent and familiar that even as far back as the time of Charles Darwin it was a recognized problem. It is a type of behavior that has to be explained. In the cases of homing in which the lost animal may have to return over a thousand miles or more, one tends to question the identity of the animal that appeared, and in reports involving comparatively short distances, ten or fifteen or twenty-five miles, one critically questions the judgment that the animal was unfamiliar with the intervening territory he had to cross on his way home. But the cases are so numerous that they present a continuum in distances involved extending up to as much as 3,000 miles. And finally, when we interview our friends and fellow scientists whose lost pets *have* returned (they maintain), a further touch of assur-

directions.) The pigeons did not adapt well to the test apparatus. The experiment with cats was admittedly poorly controlled and no reliable conclusions can be drawn.

ance, of good faith, and confidence in judgment is added. Thus eventually the accounts of the homing of these dogs and cats tend to lose a great part of the uncertainty at first attached to them. One begins, then, to wonder how science could so long have avoided consideration of this challenging material.

In these stories of the homing of dogs and cats, the animals are, for the most part, transported from home either by train or automobile. Those cases occurring in recent years involve the latter more often than the former. There are enough instances, however, in which the animal was crated and transported in an express car by rail or a closed express truck that there is no lack of material in which visual awareness of direction taken was impossible. In many of these cases in which the animal was transported from home by automobile, investigation brings the comment that the animal slept most of the way or lay on the back seat without looking out (or in the case of cats) traveled in a box on the back seat or in a trailer. In a very few cases (the most notable being that of Bobbie, the collie belonging to the Brazier family of Silverton, Oregon), there are records that the dog returned by a different route from that on which he had been taken from home, but in most cases no record is available of the homing route.

Perhaps the most interesting generalization, from the point of view of psi in animals, that can be drawn from this type of homing material is the fact that it does not seem to matter what the distance is. Judging by the relation of number of cases to the distances covered, it appears to be unimportant how far away the animal is when he starts home. There are approximately as many of these cases that involve distances over 100 miles as there are for short distances. With case material, however, we cannot make reliable comparisons; in this instance there is risk that the longer distance cases are the more likely to be reported. We can say, though, that there are enough long-distance cases to suggest that distance can hardly be an effective barrier; on the surface at least that certainly looks more like the operation of extrasensory perception than sensory. In any case, the problem of how the animal finds its way back over new and strange terrain covering hundreds and in many instances thousands of miles is something that would in itself justify consideration of psi capacity as a legitimate working hypothesis.

There is, however, a still more pertinent group of case reports on the behavior of dogs and cats which, if they are true, leave little room for doubt as to their interpretation. These have been given the tentative name of *psi-trailing* cases. They are cases in which the animal, left behind when his human companion or family go to a more or less distant location without him, somehow finds his way to them. This type of case is illustrated by the report of Tony, a dog of mixed breed belonging to the Doolen family, formerly of Aurora, Illinois, and now of Lansing, Michigan. When the Doolens left Aurora they gave Tony away, but six weeks later he appeared in Lansing, excitedly approached Mr. Doolen on the street, and was recognized. Identity was established by the collar which Mr. Doolen had bought in Aurora and had cut down to Tony's size. A right-angled notch had been cut for an extra hole. Both the Doolen family (of four) and the Aurora family who gave them Tony as a pup recognized the dog, and Tony's behavior confirmed his identity. He arrived labeled with license tags from Jackson County through which he would have passed. Apparently he had been "legally" adopted en route.

In these modern cases of psi-trailing the family naturally travels by train, automobile, or plane; accordingly the trailing can hardly be credited to the animal's olfactory sense. In many cases the trailing is not done for some days or even weeks and months after the departure of the family, and any kind of trail that could have been left would be old. Some reports involve only comparatively short distances such as six, nine, eleven, and thirty miles, and in such cases there is no reasonable question about the identification of the animal. But with less than fifty miles there is always some question whether chance alone will not explain the few examples which are successful. Larger numbers of animals may be wandering off in wrong directions and may merely be less fortunate than those that just happened to take the right direction. In other cases, however, the distances involve weeks or months of travel; then, because of the lapse of time, and to some extent because of the incredibility of the feat, the question of identity is very important—and sometimes very difficult.

Over a dozen of these more clear-cut trailing cases have already been culled from the larger stock of homing reports and, for the most

part, they appear to involve what in ordinary affairs would be considered acceptable standards of reporting. People of good standing in the community report that their dog or cat, as the case may be, followed them and found them after they moved and that they are entirely convinced it is their dog or cat. There is, in one case, a familiar dog collar still on the animal, or in another a special red tuft of hair under the cat's chin, or a pair of white spots on the black cat's belly. On any ordinary question in science an accumulation of a great deal of confirmatory case material of this type, drawn from independent sources and checked by personal interview, would undoubtedly suffice.

On the question of psi in animals, of course, we need not attempt to focus the issue solely on any one of the branches of evidence surveyed. These more impressive trailing cases, however, do at least serve to point up the inquiry more acutely than any other material. There is no question of memory of the trail, for the trail is entirely new; not even a magnetic theory would help the animal to know where his human companions have gone. All his sensory functions are ruled out in the explanation, because there is nothing known that could serve as a stimulus. If he does know or is guided by the location of his human friends, the perception or awareness of this fact would surely be extrasensory. There is little need to debate this point if even in a single case the identity of the animal is established sufficiently well, and error, hoax, and fabrication are excluded. It does not matter how many other dogs and cats do not follow the family, or how many are lost, or whatever further questions might or might not be raised. We have the clear-cut, definite starting place from which we shall want to take off on a wide range of secondary questions concerning psi in animals.

Here again, however, is a point where we can well afford to suspend judgment, especially since further inquiry is being made. Other cases are being collected and studied, and the strength of the evidence grows with the variety and number of examples. From the information gained, some experimental tests may develop. In fact, some of these same animals may be used in trailing experiments. This is merely to follow the pattern of Professor Herrick in his study of the homing ability of the cat. Finding that his pet cat returned when taken about five miles away, he immediately set to

work to take cats out in various directions from home and release them. He found that they would return from all four compass directions.

It is a good question whether, in searching for evidence of psi in animals, it is not limiting the scope of inquiry too much to concentrate on direction-finding. As Thorpe has pointed out, even in the act of homing, there is more than direction-finding. An element of home perception or place perception is involved in addition to the awareness of orientation or direction which is necessary to maintain a course toward home. We must ask too what it is that serves as the stimulus in different types of homing. In the anecdotal material some animals will, when lost on a move from home to a new place, return home, at least first. But there are cases on record in which the animal, similarly lost, shows up eventually at the new location whether or not he first went to the old home. Other cases are known in which the animal returns to the old home, especially to give birth to young, preferring to leave its family in the new situation. In the return of birds separated from home at the nesting stage the homing target is the nest, but in the case of the homing pigeon its homing behavior is built up around feeding, although there is unquestionably more than food-seeking involved in the homing pigeon's impulse to return. It is probably of the greatest importance to consider the motivating factor as a part of the design of a study of homing, just as, certainly, it is essential in the search for psi operations.

While we are suspending judgment on the question of whether psi is appreciably involved in the long-distance direction-finding in animals, it would be well to carry out a more direct frontal attack on the problem and do a certain amount of exploratory searching for animals that can, under controlled test conditions, give evidence of ESP in the location of food or in some other choice-response test. It is not easy to control such tests without so circumscribing and artificializing the animal's behavior that the delicate operation to be investigated is in danger of being obscured; but the development of methods is a part of this stage of the research. We have in the background the suggestive work of Bechterev on telepathy in dogs and that of J. B. and L. E. Rhine on the horse Lady.

Several zoologists have indicated a readiness to undertake adap-

tations of their present experiments to make a specific test of ESP on their particular animals. ESP is so elusive, however, and the conditions for its successful demonstration so poorly understood that in searching for it, especially in new species, it would not be wise to design elaborate experiments at the start. Rather, it is the simple, exploratory, easily adapted test that is called for to discover first the best place and method for making a more elaborately designed beginning. Above all, it is advisable as a practical working procedure to begin with types of behavior or capacities already known that suggest the operation of ESP. Such beginnings as are being made with animal experiments by parapsychologists themselves must of necessity be groping and guarded for a time. It is no matter for quick results and hasty conclusions. The more, too, the research project is shared with zoological co-workers experienced in this area of problems, the better it will be. There is for that matter no scientific need for a sharp division of the problems involved since everyone will want to know as exactly as he can just how far the psi type of explanation applies, if it applies, to these unknowns in animal behavior.

* * * * *

We shall emphatically not be satisfied in this study of animals merely to search for evidence of behavior that cannot be explained on the basis of sensory perception. Extrasensory perception in human beings is not *merely* perception of external objects or of activities that is not yet accounted for on sensory grounds. It is more than a negatively defined phenomenon. Truly enough, as with all perception, we do not understand much of what takes place, and we are still in search of more of the positive identifying characteristics of ESP in man. One positive identifying characteristic of ESP is the familiar fact that ESP in man is an unconscious function, but unfortunately it is one that we cannot verify in animals. The suggestion might be made, however, that since ESP is unconscious in man (and sensory perception conscious) we should then *expect* to find ESP in lower orders of living things as a presensory mode of orientation.

On the nonpsychological side the most conspicuous feature of ESP, of course, is its failure to show the kind of relation to space and time that we have come to expect in all other activities that can

be tested. So far as independence of space is concerned ESP fits the requirements for distance homing and migration perfectly, and serves as well for night as for day travel. The fact that generally stands out over the whole survey of homing and migration, both in experimental and in field and anecdotal material, is that the longer distances do not hamper the returning animal so far as finding his way is concerned. Some observers of bird homing even state that orientation on release as well as speed of return is better at longer distances than at shorter. The natural hazards to the birds, which do increase with distance, make such comparisons difficult, especially in terms of percentages of return.

Not enough is known about nature as yet to furnish an intelligible hypothesis as to how a transported bird or a lost dog or cat can actually be as close perceptually to its goal when it is a thousand miles away from it as it is at ten miles or one, but we already know on the basis of the studies of psi in humans that it will be necessary to find the principles to explain just such an effect.

Little is known too about time perspective in animals. There is some interest among students of homing in the response of certain animals to changes that involve time, such as the position of the sun. The animal is figuratively said to be "carrying a clock," but even if this be taken at its full value, it would not necessarily mean that the animal had a time perspective that would, for example, involve an appreciation of futurity. It is true that among the case reports of unusual behavior in animals there are a fair number of cases in which the reaction is taken to be premonitory. These cases might be regarded as sufficient to suggest experimental tests; but the kind of studies thus far made on animal behavior would hardly have brought out anything on this point.

In the search for earmarks of psi in animals based on the presence of that capacity in humans, there is, of course, the great handicap that even if the abilities were identical, the manner of testing, the kind of evidence, the wide range of other differences involved, would make comparison very difficult. To find anything comparable we must turn to the kind of tests most nearly approximating the laboratory procedures used with human subjects. These are the experiments with mice carried out by Neuhaus and by Krüger and Vogelberg. Here we do find some similarities that for the present should

only be taken as suggestive until closer statistical comparison can be made and the more comparable kinds of series of data laid side by side for closer judgment.

On the surface, at any rate, there do appear to be a number of interesting similarities. First, there is the feature of extreme elusiveness. The workers mentioned found that they would only catch the effect (response to direction) in terms of marginal deviation significant only in fairly large numbers of trials. Second, they observed that they could get their best effect by limiting the number of trials to two per day, with about six hours between. They were using hunger as the motive, and apparently the necessary pitch of motivation was lowered too much if more trials were given. Third, some animals did not show the ability at all, some to an insignificant degree, and a persistently high-scoring animal was rare. All these points are reminiscent of ESP in human subjects as far as they go.

The comparison may even be carried a little further and a fourth point added. As we have already mentioned, there are indications, particularly in the work of Krüger and Vogelberg, which is more recent, of a change of trend (even amounting sometimes to a reversal) on the part of the animal after a certain number of trials in which some progress had been made in the direction of finding the right way out of the apparatus without error. Such shifts of scoring trends would be strange to the realm of learning based on sensory functions, but it is quite in order in the course of a series of tests in ESP in human beings. As a fifth point, the same is true of a consistent avoidance reaction shown by some mice and rats in consequence of which they produce fewer successes than chance alone would give. And sixth, there is a consistent missing tendency (that is, a tendency to give the wrong response consistently). Both this consistent avoidance and consistent missing were found by Krüger and Vogelberg in some of their mice and rats. Seventh, and finally, some tendency is found too for a high rate of success to be made at the start followed by a drop to chance scoring, an effect very typical of ESP in the human laboratory.

It will be better to emphasize the encouragement these suggestions give to the continuance of the experiments than to attempt to attach a given weight to them at this stage. Few, perhaps, will

question the view that their weight is already enough to justify continuance, and to merit for such work a fair degree of priority.

For the excursion into the question of psi in animals (anpsi) taken as a whole certain things are discernible. We have passed the point where there is any question that the investigation is warranted. There is clearly a lot to explain in animal behavior that could be due to ESP and is not as yet otherwise accounted for. We have even reached a second stage, that at which we find behavior that can be accounted for by nothing else that is *known and experimentally verified* except extrasensory perception. This is not to say that the evidence itself is unreservedly acceptable and final, nor that, even if it were, ESP is necessarily the explanation. It is the only *known* principle that is adequate. The psi-trailing cases are, as far as anecdotal material can be, an example. The supposition required to explain by sensory means such findings as those of Matthews (and now of Kramer also) on pigeons are pretty tall and speculative but further experiments should clear them up.

The anpsi problem is now recognizedly part of an area of exceptional complexity. The sensory modes of perception in animals are being further explored with great ingenuity and skill, and other studies involving cognitive and adaptive behavior are widening the perspective with which the anpsi hypotheses will need to be viewed. But if in a few instances we find a clear case for the hypothesis of psi in animals, it will, there is little doubt, be given its due consideration in future experimental studies of animal behavior.

It is important not to lose sight of the larger framework in which this biological branch of psi research belongs. We are extending a program already centered in man, in the problem of his place in nature, seeking to find out from this comparative study more about the fragments we have already got from man himself. The growing appreciation of the unity of science and of the central integrative value of understanding man himself will make it easier for parapsychology to obtain the extensive cooperation greatly needed to carry through so large an undertaking.

Selected Bibliography

Alexander, C. *Bobbie, A Great Collie.* New York: Dodd, Mead, and Co., 1926.

Bechterev, W. "Direct influence" of a person upon the behavior of

animals. *J. Parapsychol.*, 1949, **13,** 166-76. (Translated and condensed from *Z. Psychotherapie,* 1924.)

BURT, W. H. *Territorial Behavior and Populations of Some Small Mammals in Southern Michigan.* Misc. Publ. Mus. Zool. Univ. Mich., No. 45, 1940, 1-58.

CLARKE, C. W. Night flying of the homing pigeon in the U. S. Signal Corps. *Nat. Hist.*, 1933, **33,** 409-18.

DROST, R. Über Vogelwanderungen in den Wintermonaten. *Proc. 6th Int. Orn. Congress,* Copenhagen (1926), 1929, 62-79.

GOETHE, F. Beobachtungen und Untersuchungen zur Biologie der Silbermöwe (*Larus a. argentatus* Pontopp.) auf der Vogelinsel Memmerstand. *J. Orn.*, 1927, **85,** 91-119.

GRIFFIN, D. R. Homing experiments with herring gulls and common terns. *Bird-Banding,* 1943, **14,** 7-33.

———. Homing experiments with Leach's petrels. *Auk,* 1940, **57,** 61-74.

———. The sensory basis of bird navigation. *Quart. Rev. Biol.*, 1944, **19,** 15-31.

GRIFFIN, D. R., AND HOCK, R. J. Airplane observations of homing birds. *Ecology,* 1949, **30,** 176-98.

GRZIMEK, B. Heimfindende Versuche mit Pferde. *Z. Tierpsychol.*, 1943, **5,** 481-97.

GUNDLACH, R. H. A field study of homing in pigeons. *J. comp. Psychol.*, 1932, **13,** 397.

———. A test of "directional sense" in cats and pigeons. *J. comp. Psychol.*, 1931, **12,** 347-56.

HARDY, A. C. Zoology outside the laboratory. *Advancement of Science,* Vol. 6, 1949.

HEINROTH, O., AND HEINROTH, K. Das Heimfindevermögen der Brieftauben. *J. Orn.*, 1941, **89,** 213-56.

HERRICK, F. H. Homing powers of the cat. *Sci. Mon.*, 1922, **14,** 526-39.

HITCHCOCK, H. B. Aerial observations of homing pigeons. (Unpublished paper presented to the Amer. Soc. of Zoologists, Dec. 30, 1950.)

HUXLEY, J. S. Natural selection and evolutionary progress. *Nature* (London), 1936, **138,** 571-73, 603-5.

KRAMER, G. Experiments on bird orientation. (Awaiting publication.)

———. Orientierte Zugaktivität gekäfigter Singvögel. *Naturwiss.*, 1950, **37,** 188-89.

———. Über Richtungstendenzen bei der nächtlichen Zungunruhe gekäfigter Vögel. *Orn. biol. Wissenschaft,* 1949, 269-83.

———. Weitere Analyse der Faktoren, welche die Zugaktivität des gekäfigten Vögels orientieren. *Naturwiss.,* 1950, **37,** 377-78.

KRAMER, G., AND ST. PAUL, U. Ein wesentlicher Bestandteil der Orientierung der Reisetaube: Die Richtungsdressur. *Z. Tierpsychol.,* 1950, **7,** 620-31.

KRÜGER, F., AND VOGELBERG, L. Versuche über die Richtungsorientierung bei weissen Mäusen und Ratten. *Z. Tierpsychol.,* 1951, **8,** 293-322.

LACK, D., AND LOCKLEY, R. M. Skokholm bird observatory homing experiments I, 1936-1937. Puffins, storm-petrels and Manx shearwaters. *British Birds,* 1938, **31,** 242-48.

LOCKLEY, R. M. *Shearwaters.* London: J. M. Dent & Sons, Ltd., 1942.

MCILHENNY, E. A. An early experiment in the homing ability of wildfowl. *Bird-Banding,* 1940, **11,** 58-60.

MANWELL, R. D. Homing instinct of the red-winged blackbird. *Auk,* 1941, **58,** 184-87.

MATTHEWS, G. V. T. (Report on release experiments with homing pigeons to appear Dec., 1951, issue, *J. exp. Biol.*)

MURIE, O. J., AND MURIE, A. Travels of *Peromyscus. J. Mammal.,* 1931, **12,** 200-209.

NEUHAUS, W. Untersuchungen über die Richtungsorientierung bei Mäusen. *Z. Tierpsychol.,* 1948, **6,** 236-61.

———. Versuche über die Richtungsorientierung mit normalen und blinden Mäusen. *Z. Tierpsychol.,* 1950, **7,** 380-402.

RHINE, J. B., AND RHINE, L. E. An investigation of a "mind-reading" horse. *J. ab. soc. Psychol.,* 1929, **23,** 449-66.

———. Second report of Lady, the "mind-reading" horse. *J. ab. soc. Psychol.,* 1929, **24,** 287-92.

RIVIERE, B. B. The homing faculty in pigeons. *Proc. 6th Int. Orn. Congress,* Copenhagen (1926), 1929, 535-55.

———. Homing pigeons and pigeon racing. *British Birds,* 1923, **17,** 118-38.

ROWAN, W. Experiments in bird migration. *Trans. Royal Soc. Canada,* 1946, **40,** 123-35.

RÜPPELL, W. Heimfindeversuche mit Staren, 1934. *J. Orn.,* 1935, **83,** 462-524.

———. Heimfindeversuche mit Staren und Schwalben, 1935. *J. Orn.,* 1936, **84,** 180-98.

———. Heimfindeversuche mit Staren, Rauchschwalben, Wendehälsen, Rotrückenwürgen und Habichten, 1936. *J. Orn.,* 1937, **85,** 120-35.

RÜPPELL, W., AND SCHIFFERLI, A. Versuche über Winter-Ortstreue an *Larus ridibundus* und *Fulica atra,* 1935. *J. Orn.,* 1939, **87,** 224-39.

RÜPPELL, W., AND SCHÜZ, E. Result of transport of fog-crows during the return migration. *Vogelwarte,* No. 1, 1948, 30-36.

SCHIFFERLI, A. Verfrachtungsversuche mit Alperseglern. *Orn. Monatsber.,* 1942, **39,** 145-50.

SCHLEIDT, W. M. Orientierende Versuche über die Heimkehrfähigkeit der Rötelmaus (*Evotomys glareolus ruttneri* Wettst.) *Z. Tierpsychol.,* 1951, **8,** 132-37.

SCHMID, B. *Interviewing Animals.* Boston: Houghton Mifflin Co., 1937.

———. Über die Heimkehrfähigkeit von Waldmäusen. *Z. vergl. Physiol.,* 1936, **23,** 592.

———. Vorläufiges Versuchsergebnis über das hundliche Orientierungsproblem. *Z. Hundeforsch.,* 1932, **2.**

SCHÜZ, E. Die Früh-Auflassung ostpreussischer Jungstörche in West-Deutschland durch die Vogelwarte Rossitten 1933-1936. *Bonner Zool. Beitrage,* 1950, **15,** 239-53.

———. Die Spät-Auflassung ostpreussischer Jungstörche in West-Deutschland durch die Vogelwarte Rossitten 1933. *Vogelwarte,* No. 2, 1949, 63-78.

———. Verfrachtete Zwischenzug-Stare kehren zum Teil in das Gebiet ihrer Jahresverbreitung zurück. *Vogelwarte,* No. 3, 1950, 192-94.

STICKEL, L. F. An experiment on *Peromyscus* homing. *Amer. Midl. Nat.,* 1949, **41,** 659-64.

SUMNER, F. B. Psychology and some things that fishes do. *Sci. Mon.,* Sept., 1939.

THORPE, W. H. Recent biological evidence for the methods of bird orientation. Symposium: a discussion on the orientation of birds in migration and homing flights. *Proc. Linnean Soc.,* 1947-48, **160,** 85-94.

WATSON, J. B., AND LASHLEY, K. S. An historical and experimental study of homing. *Carnegie Instn. Publ.,* 1915, **7,** 7-60.

WODZICKI, K., PUCHALSKI, W., AND LICHE, H. Untersuchungen über die Orientation und Geschwindigkeit des Fluges bei Vögeln. III. Untersuchungen an Störchen. *Acta Orn. Mus. Zool. Polon.,* 1938, **2,** 1-20.

———. Untersuchungen über die Orientierung und Geschwindigkeit des Fluges bei Vögeln. V. Weitere Versuche an Störchen. *J. Orn.,* 1939, **87,** 99-114.

WODZICKI, K., AND WOJTUSIAK, R. J. Untersuchungen über die Orientation und Geschwindigkeit des Fluges bei Vögeln. I. Experimente an Schwalben. *Acta Orn. Mus. Zool. Polon.,* 1934, **1,** 253-74.

WOJTUSIAK, R. J. Polish investigations on homing in birds and their orientation in space. Symposium: A discussion on the orientation of birds on migration and homing flights. *Proc. Linnean Soc.* (1947-48), **160,** 99-108.

WOJTUSIAK, R. J., AND FERENS, B. Homing experiments on birds. VII. Further investigations on the velocity of swallows and on the role of memory in their orientation in space. *Bull. Acad. Polon. Sc. Cracovie,* B, 1946, **2,** 135-64.

———. Homing experiments on birds. VIII. Observations on the attachment to the nest, the age and the faculty of orientation in space of chimney swallows. *Bull. Acad. Polon. Sc. Cracovie,* B, 1946, **2,** 165-67.

———. Untersuchungen über die Orientation und Geschwindigkeit des Fluges bei Vögeln. IV. Heimkehrgeschwindigkeit und Orientierungsart bei den Rauchschwalben. *Bull. Acad. Polon. Sc. Cracovie,* B, 1938, **2,** 173-201.

WOJTUSIAK, R. J., WODZICKI, K., AND FERENS, B. Homing experiments on birds. VI. Investigations on the tree and house sparrows. *Bull. Acad. Polon. Sc. Cracovie,* B, 1946, **2,** 99-106.

———. Untersuchungen über die Orientation und Geschwindigkeit des Fluges bei Vögeln. II. Weitere Untersuchungen an Schwalben: Beeinflussung durch Nachtzeit und Gebirge. *Acta Orn. Mus. Zool. Polon.,* 1937, **2,** 39-61.

Parapsychology Laboratory
Duke University
Durham, North Carolina

INTROVERSION-EXTRAVERSION RATINGS IN RELATION TO SCORES IN ESP TESTS

By Betty M. Humphrey[1]

ABSTRACT: The purpose of the analysis reported here was to determine whether ESP scoring level is related to the introversion-extraversion ratings of the Bernreuter Personality Inventory. Three ESP experiments were used as the basis of the analysis.

In the first, 14 subjects each made 100 runs of GESP card tests. They were then given the Bernreuter, on the basis of which they were separated into extraverts and introverts. When the ESP scores of both groups were compared it was found that 1,000 runs done by the extraverts gave an average ESP score that was significantly higher than chance, and 400 runs done by the introverts gave a below-chance average. The difference between the two averages was statistically significant.

In a further investigation the data of two other experiments were analyzed. Nineteen extraverts from these two series made 380 ESP runs with an average run score of 5.27, which would be expected by chance only about once in 125 such series. The 21 introverts did 400 ESP runs with a score average which was below chance expectation, but not significantly so. The difference between the average scores of the two groups of subjects was significant with a probability of .005.

The consistency of this separation of high and low ESP scorers on the basis of their extraversion-introversion ratings was of a high order: 74 per cent of the extraverts in the two confirmatory series gave positive ESP scores and 71 per cent of the introverts gave scores below the expected chance average.—Ed.

In recent years a line of research has developed that promises to throw some light on the nature of extrasensory perception (ESP) and its relations to more familiar concepts in psychology. The object of these investigations has been to discover whether any personality characteristics are consistently associated with different levels of scoring in ESP tests. The finding of relationships between ESP ability and certain personality characteristics should not only give us a greater degree of control over the demonstration of ESP, but should also provide an increased understanding of its nature.

In a number of experiments dealing with the relations of ESP performance and personality ratings, Dr. Gertrude Schmeidler found

[1] This paper was read at the annual meeting of the American Psychological Association in Chicago on September 1, 1951.

The study here reported is part of a larger project which is being carried out at the Parapsychology Laboratory with the aid of a research grant from The Rockefeller Foundation, whose support is gratefully acknowledged.

that subjects rated "well-adjusted" on the basis of their Rorschachs (**8, 9**) gave significant ESP scores, whereas the "poorly-adjusted" subjects as a group gave ESP scores near the expected chance average. Those subjects in Schmeidler's experiments who were open-minded toward the possibility of the existence of ESP made positive scores in ESP card tests while subjects who denied the possibility of ESP made significantly negative ESP scores (**10**). In several of my own researches in which interest inventories were used (**2, 3**), certain patterns of likes and dislikes were found to be characteristic of high-scoring ESP subjects; low scorers in the ESP tests showed a different pattern of likes and dislikes. It was also found that high- and low-scoring ESP subjects could be separated on the basis of certain form qualities shown in their freehand drawings (**5**).

Since these researches are mentioned here briefly only to serve as an introduction to the present report, details of the experiments will not be given. Suffice it to say that the results of the various series in recent years have led to a re-examination of several early experiments in which the Bernreuter Personality Inventory was given the subjects. The previous experimental findings suggested that performance level in ESP tests might be related to the Bernreuter Introversion-Extraversion Scale. According to Bernreuter, persons scoring high on this scale "tend to be introverted; that is, they are imaginative and tend to live within themselves. . . . Those scoring low are extroverted; that is, they rarely worry, seldom suffer emotional upsets, and rarely substitute day dreaming for action."[2]

In the first experimental series in which the Bernreuter was used, a difference in the ESP scoring rates of extraverts and introverts was found; and on the basis of this finding two other experimental series were analyzed to see if the first result could be confirmed. Details will be given below.

The First Series

Procedure

The first experimental series was carried out by me in 1938 at Earlham College, Richmond, Indiana (**4**). In all the tests the

[2] Robert G. Bernreuter, *Manual for the Personality Inventory*. Stanford University Press, Stanford University, California, 1935.

subjects were asked to identify the symbols in a regular pack of ESP cards. In this pack there are five suits—crosses, squares, circles, stars, and waves. A deck consists of five of each symbol—25 cards in all. When a subject tries to identify the order of the 25 symbols in the pack, he is expected to average five hits by chance alone.

In these tests two experimenters were present at each session: one, the sender, or agent, whose task was to look at each card in succession as the subject attempted to call it, and another who recorded the subject's choice on each trial. The experimenter who served as agent also made a record of the true card order. In all tests the cards were kept from the subject's view by a wooden screen and, as an additional precaution, the subject was always seated across the room with his back to the experimenters. Not until all 25 trials in the pack had been made was the subject ever informed of his success. After each "run" or series of 25 calls through the pack, the experimenters shuffled and cut the cards behind the screen before another "run" was made.

In all, 14 subjects participated in the experiment, each subject calling through 100 ESP packs, a total of 2,500 trials for each person. Ordinarily from 10 to 20 runs were made by a subject at a session. At the completion of his 100 runs, each subject was asked to fill in the Bernreuter questionnaire.

When the data were re-analyzed for the present study, the following procedure was used. Subjects receiving a score of less than 50 per cent on the Bernreuter Introversion-Extraversion Scale were arbitrarily labelled "extravert"; those subjects having a score of more than 50 per cent were rated "introvert." The ESP scores of the subjects within each group were then pooled and averaged. The mean scores per run of 25 ESP trials for the two groups were then compared with each other and with mean chance expectation.

Results

Of the 14 subjects, 10 were rated extravert and 4 introvert. In the 1,000 runs of 25,000 trials of the extraverts there were 261 hits more than the number expected by chance. The average number of hits per run for this group is 5.26 and the critical ratio is 4.13. Thus the probability associated with the ESP score of the extraverts is approximately .00003.

The 400 runs, or 10,000 ESP trials, of the introverts gave only one hit more than the number expected by chance. The average run score of the introverts is 5.0025, which is almost exactly the expected chance average.

The critical ratio of the difference between the mean ESP scores of the extraverts and introverts is 2.19, which has a probability of approximately .03.

On the basis of these results, the following hypothesis was suggested: extraverts, that is, subjects scoring below the 50th percentile of the Bernreuter scale, will score significantly higher on ESP card tests than will introverts, that is, subjects with scores above the 50th percentile on the Bernreuter scale.

In following up this suggested relationship, I chose for analysis two later series of ESP tests which were closely comparable to the 1938 series in general conditions and results. The series analyzed both consisted of card-calling tests in which there was evidence of the operation of ESP. The description of these series follows.[3]

The Two Test Series

Procedures

One of the two series used to test the hypothesis of a relation between the introversion-extraversion scale and ESP scores was carried out by Dr. J. G. Pratt and me at Duke University in 1941 (**6**). The other ESP experiment was conducted in 1948, under my direction, by Miss Ann Lawrence, then a senior psychology student at the College of William and Mary (**7**). The two experiments were similar in that each subject called 20 runs of ESP cards, usually ten runs per session.

In the Pratt-Humphrey series the ESP test was of the type commonly called "precognitive"; that is, the subjects were asked to predict the order of cards in a pack which would be shuffled and cut *after* the subjects' choices had been made. Briefly the procedure was as follows: the subject was given a record sheet containing space for 10 ESP runs or 250 calls. He was asked to fill in the sheet, trying to predict the order of ESP symbols that would be

[3] The evidence for ESP in these two series is based upon significant position effects. In the Pratt-Humphrey series two quarters of the pooled record pages were significantly different (**1**). In the Lawrence series the scoring rate of the first ten runs of all subjects was significantly different from the scoring rate of their second group of ten runs.

written in later on a corresponding record sheet. The subject was told that only after his record sheet had been filled in would the true symbol order be determined by a method of random selection. He was then left alone to complete his task, after which he sealed his record in an opaque envelope and took it to the two experimenters for the checkup.

The order of the target symbols was determined in this manner: a pack of ESP cards was shuffled thoroughly by one of the experimenters. A random cut of the pack was obtained by a complicated method based on the throw of three 8-sided dice and an intervening cut of a pack of shuffled numbered cards. This method will not be described in detail here for lack of time; it was a routine method of cutting an ESP pack in a random manner.

This procedure was followed in determining the symbol order for each run of 25 trials. Together the experimenters recorded the symbols on a record sheet similar to the subject's. When the ten-run target sheet had been filled in, the envelope containing the sheet of subject's calls was opened and the two symbol orders compared by the experimenters. The subject was allowed to watch the checkup if he desired.

In the experiment carried out at William and Mary by Lawrence, a "clairvoyance" test of ESP was used; that is, the subject was asked to identify the symbols on ESP cards whose order was unknown at the time of the test. The procedure was as follows: the subject and experimenter were seated at opposite sides of a table with a 24" x 28" wooden screen on the table between them. The experimenter shuffled and cut a pack of ESP cards behind the screen, and then, holding the first card slightly apart from the pack, she asked the subject to record his impression of the card symbol. The experimenter did not look at the faces of any of the cards until the end of the run. When the subject had written down his choice for the first card, he tapped a signal key on his side of the screen thereby flashing a light beside the experimenter. At this signal the experimenter placed the first card in a separate pile and picked up the second card. After the signal at the start of the run, there was no further verbal communication between the two until the entire pack of 25 cards had been called; the subject was told that each time he flashed the experimenter's light a new card

would be held in position. Thus the subject was allowed to proceed at his own rate of speed through the run. At the end of the 25 calls, the subject folded his record sheet and gave it to the experimenter, who placed it under her record pad. Then, with the subject watching, she made a record of the true card order on her own record sheet, and the two columns of symbols were compared for hits. Ten such runs were made in a session. Unchecked carbon copies of all data were sent to Duke University for an independent count of the hits. The Bernreuter Personality Inventory was given to the subjects after their second session.

Results

There were 19 subjects in the Pratt-Humphrey series and 21 in the Lawrence series. Of the total of 40 subjects 19 were rated extravert and 21 introvert.

The 19 extraverts called 380 runs of ESP cards with a total of 104 hits more than expected by chance. The average run score of this group is 5.27, which compares closely with the average of 5.26 obtained by the extraverts in the 1938 series. The critical ratio of the 19 extraverts in the test series is 2.67, which has a probability of approximately .008. (See Table 1.)

Table 1

ESP Scores for the Introverts and Extraverts in Two ESP Experiments

Series	Extraverts				Introverts				CR_d
	N	Runs	Dev.	Av. Score	N	Runs	Dev.	Av. Score	
Pratt-Humphrey..	10	200	+56	5.28	9	160	−34	4.79	2.32
Lawrence.....	9	180	+48	5.27	12	240	−18	4.93	1.73
Total.....	19	380	+104	5.27	21	400	−52	4.87	2.82
		CR=2.67 P= .008				CR=1.30 P= .19			P=.005

The 400 runs, or 10,000 trials, of the 21 introverts gave 52 hits fewer than the number expected by chance. Their average run score was 4.87, where 5.00 is the expected chance average. The critical ratio for this group is 1.30, which is not statistically significant.

When the means of the two groups are compared, the difference is significant, with a critical ratio of 2.82 and a probability of less than .005.

The test series therefore confirms the earlier finding that extraverts as measured by the Bernreuter do score significantly higher in ESP card tests than do introverts.

The critical ratio just cited shows that there is indeed a significant difference between the ESP scores of a group of introverts and those of a group of extraverts. But how many individual subjects followed the trend reflected in the group totals? A count of these shows that 74 per cent of the extraverts gave positive ESP scores and only 21 per cent of them gave ESP scores below mean chance expectation. (Five per cent of the extraverts gave scores at the expected chance level.) Of the introverts 71 per cent made negative scores (that is, scores below the chance average), while only 24 per cent gave more hits than expected by chance. (Five per cent gave the expected chance score.) The total result is not, therefore, due to a few outstanding subjects in each group but reflects the scores of a majority of the subjects.

In order to test whether or not positive and negative deviations on ESP tests are significantly related to introvert-extravert ratings, a fourfold contingency table was made using the figures for the subject groups from which the above percentages were obtained. The number of extraverts who obtained positive ESP deviations on the one hand, and those who had zero or negative deviations on the other, were compared with the number of introverts who gave positive and those who gave negative or zero deviations.

	ESP Deviation		
	+	0, −	Total
Extraverts	14	5	19
Introverts	5	16	21
Total	19	21	40

$\chi^2 = 8.05$
d.f. = 1
P = .005

The chi square based on this fourfold table (after Yates' correction) is 8.05, which with one degree of freedom is significant at the .005 level. This measure indicates that there is a significant association between positive ESP deviations and extraversion on the one hand, and negative ESP deviations and introversion on the other.

One further analysis of the data was made to test the degree of relationship between ESP scores and introversion-extraversion ratings. A regression study showed that the relation between these two variables is linear. The coefficient of the regression of Bernreuter scores on ESP scores is −2.38, which is significant, with a probability of less than .001. This regression coefficient means that for every one point rise in ESP score we expect a drop of 2.38 on the Bernreuter Introversion-Extraversion Scale.

Discussion

One would be justified in inquiring why, in 1951, we are using the Bernreuter for personality assessment when so many newer, more adequate tests are available. The main reason is that the present research was begun in 1938 when the Bernreuter test was enjoying great popularity among psychologists. In spite of the fact that usage of the Bernreuter has declined in recent years, it does appear to have "considerable validity as a research instrument," as Super concluded in 1942 (**11**). Regardless of its shortcomings, we are still confronted with the fact that by the use of the Bernreuter introversion scale, subjects in ESP tests have been separated into two significantly different groups: the Bernreuter has empirical value for ESP research. Without some basis such as the Bernreuter for separating subjects with conflicting ESP scoring trends, the presence of ESP in these series might have gone undetected; the *total* deviation of the test series was not statistically significant (CR <1).

The Bernreuter not only separates high- and low-scoring subjects in these series, but does so with a high degree of consistency: over 70 per cent of the extraverts gave positive ESP scores and over 70 per cent of the introverts gave negative ESP scores. Thus far in ESP personality research, no other measure of personality has distinguished high and low ESP scorers with this degree of ac-

curacy. For example, one of the most fruitful lines of research has involved the Rorschach, and yet the best combination of Rorschach factors has correctly placed at most about 66 per cent of the subjects into high- or low-scoring ESP groups (**8**).

These facts do not, however, lead to the conclusion that the Bernreuter scale is superior to the Rorschach or other measures which have been used as instruments for research in ESP. There were differences in experimental conditions and procedures that may account for the apparent superiority of the Bernreuter. One notable difference is that in the experiments involving the Bernreuter, the subjects were tested individually, while in the series in which the Rorschach and other measures were used, groups of subjects were tested all at the same time, usually in the classroom situation. Previous experience in ESP research indicates that the individual test condition is more favorable than the group situation for producing ESP results. In the few cases in which a personality rating has been applied to data obtained in both group and individual tests, the rate of ESP scoring found in the latter was higher. Thus the results with the Bernreuter may be more consistent than those with the other tests because they were obtained under more favorable ESP conditions.

The higher degree of separation found with the Bernreuter might also be due to the fact that the subjects in the Bernreuter series were given more ESP tests than were those in the series involving other personality ratings. In the Rorschach series subjects customarily called nine packs of ESP cards; in other personality-ESP experiments subjects made calls for from two to four ESP packs. In the test series using the Bernreuter, however, each subject called twenty packs of ESP cards. The data obtained from the latter subjects, then, may constitute a more adequate sample from which to infer the subject's ESP scoring level. This problem of determining how many trials constitute an adequate sample for estimating a subject's customary ESP scoring level cannot be answered on the basis of the data now available. Analysis of the present series does show, however, that if the subjects had called only ten packs of ESP cards each, the difference between extraverts and introverts would not have been significant. Moreover, if we consider only the first four packs called by each subject, there is no difference at all between the

scores of the introverts and extraverts. These results indicate the need for further investigation of the problem of the optimum size of ESP samples for personality research.

It is rather difficult to discuss in a few words the relation of the Bernreuter findings with those obtained from the use of other measures of personality. The general picture of ESP-personality relationships drawn from previous researches is that ESP functions best in the "normal," well-adjusted, confident, nonconstricted personality. The fact that extraverts give ESP scores significantly higher than those of introverts fits in well with this picture, since the Bernreuter scale is merely another means of measuring what has been termed social adjustment.

The ESP-personality relation, however, is much more complicated than these simplified statements imply. The results from the various isolated experiments are not easily unified into one coherent picture. But they have been useful in that they have enabled us to predict with a fair degree of success a subject's ESP score from a knowledge of his rating on a given personality test.

The time has come now, however, to carry out an intensive study of the personality characteristics of ESP subjects, using not only a battery of personality tests, but also a battery of ESP tests. Two beneficial results could be expected from such an investigation: increased understanding of how ESP fits in with personality, and a higher degree of precision in predicting ESP scores as a result of combining various ratings of personality.

References

1. Humphrey, B. M. Further position effects in the Earlham College series. *J. Parapsychol.*, 1945, **9,** 26-31.
2. ———. Further work of Dr. Stuart on interest test ratings and ESP. *J. Parapsychol.*, 1949, **13,** 151-65.
3. ———. A new scale for separating high- and low-scoring subjects in ESP tests. *J. Parapsychol.*, 1950, **14,** 9-23.
4. ———. Patterns of success in an ESP experiment. *J. Parapsychol.*, 1943, **7,** 5-19.
5. ———. Success in ESP as related to form of response drawings: I. Clairvoyance experiments. *J. Parapsychol.*, 1946, **10,** 78-106.

6. HUMPHREY, B. M., AND PRATT, J. G. A comparison of five ESP procedures. *J. Parapsychol.*, 1941, **5,** 267-92.
7. LAWRENCE, A. Unpublished manuscript.
8. SCHMEIDLER, G. R. ESP performance and the Rorschach test. *J. Soc. psych. Res.*, 1950, **35,** 323-38.
9. ———. Rorschach variables in relation to ESP scores. *J. Amer. Soc. psych. Res.*, 1947, **41,** 35-64.
10. ———. Separating the sheep from the goats. *J. Amer. Soc. psych. Res.*, 1945, **39,** 47-50.
11. Super, D. E. The Bernreuter Personality Inventory: a review of research. *Psychol. Bull.*, 1942, **39,** 94-125.

Parapsychology Laboratory
Duke University
Durham, North Carolina

MINOR ARTICLES

A COMPARISON OF TWO SUBJECTS IN PK

By Paul and Christiane Vasse

ABSTRACT: the dice-throwing experiments reported here originated in an attempt to direct into a more measurable form what appeared to be spontaneous psychokinetic effects associated with one of the authors (C.V.). C.V. and her husband, P.V., threw 480 runs with a pair of dice, the target part of the time being high dice (a total of eight or more for the two faces) and part of the time low dice (a total of six or less). C.V. scored significantly in both categories while P.V. scored at the chance level. The dice were recorded in sets of three runs down the page. Analysis of the results showed that C.V. had a statistically significant decline in her scoring in these three runs within the set. P.V., on the other hand, had a statistically insignificant incline.

The authors suggest that C.V.'s results in these PK tests may be related to spontaneous PK effects associated with her and to her apparent success in some tests on influencing the growth of seeds.

This is the first article by the Vasses to appear in the JOURNAL. Dr. Vasse is a physician in Amiens, France.—Ed.

THE EXPERIMENTS in psychokinesis which we are reporting were carried out with the technique of dice-throwing, but our interest in psychokinesis originated in what appeared to be spontaneous psychokinetic effects associated with one of us, C.V. These odd and unexplainable physical phenomena began to manifest themselves soon after our marriage and they led P.V. to make a study of such matters. His attention was attracted by the work of Montandon (1) to the idea of utilizing whatever influence it was that was causing these phenomena by directing it to the production of an observable effect under better conditions of control.

As a result of this turn of thought we tried some experiments in which C.V., as the principal subject, attempted to influence the rate of germination and growth of seeds. We planted some seeds on a plate and used one half as the experimental side, leaving the other half as a control. In a series of tests it was observed that the experimental part of the plate produced a higher rate of germination and taller seedlings than the control.

At this point Dr. J. B. Rhine, with whom we had been in correspondence on the matter for some time, made the suggestion that we try to influence the phototropism of the young corn seedlings with which we had been experimenting. The results suggested that

it may be possible to counteract the phototropic influence to a measurable extent.

Although both P.V. and C.V. tried to influence the growth of seedlings it was quite clearly C.V. who had the more marked effect. P.V. had some success but it did not persist as it did with C.V. We have given an account of these experiments in the *Revue Métapsychique* (3).

Satisfied that we were working with some force or factor that needed further study, we endeavored to introduce further precautions against possible error. It seemed that the methods developed by the Duke Laboratory, based on the technique of dice-throwing, offered a better measure of the possible influence involved in the seedling test. It was a reasonable assumption, at any rate, since many subjects at Duke and other laboratories had been found to possess PK ability. Accordingly, we undertook to carry out a comparative PK test with dice with the general purpose in mind of observing whether this statistical and experimental method of measurement would differentiate between C.V. and P.V. as the seed test had done and whether something could be measured that could account for the fact that the odd physical occurrences took place only in C.V.'s presence and never occurred to P.V. when he was alone.

It appeared that the experiment conducted by Margaret Pegram Reeves with high dice and low dice as targets (2) offered the most suitable procedure for our purposes. Accordingly, with a few modifications, this procedure was followed.

Procedure

We used throughout the experiment a pair of ordinary ivory dice with excavated faces, such as are commonly used for games. They were shaken in a leather dice cup, the sides of which were channeled on the inside so that the grooves formed elongated **X**'s. They were thrown onto a piece of woolen carpet placed on a table top and surrounded by books to keep the dice from rolling to the floor. Twelve throws of the two dice made a run.

The records were kept in a notebook, the odd-numbered pages being reserved for C.V. and the even pages for P.V. We took turns as subject and assistant, the assistant doing the recording. Each page contained two columns of three runs each; i.e., six runs

to a page. Almost always one column was thrown for high dice (a sum of the two faces equal to 8 or higher) as the target and one for low dice (a sum of 6 or lower), though occasionally P.V. did a whole page for the same target. The choice of whether we should do high or low dice first on the page was left to the preference of the subject. We kept an account of the targets used at each session, however, in order to equalize the final number of each which were carried out. The scores were noted at the end of each session in a special column reserved in the middle of the page.

On some days we did one page in our notebook and on other days when we had more time or if we felt like it, we did more than one page. We recorded in the notebook the dates, hours, and certain other information, particularly the state of the subject's health, the atmospheric conditions, and whether spontaneous PK phenomena were produced on that day.

When C.V. was the subject she almost always alternated her choice of target, and P.V., acting as assistant, knew what this target was. When P.V. was subject, however, he sometimes threw several sessions for the same target because he hoped thereby to "baffle the enemy." This "enemy" was the recorder, C.V., who could not prevent herself from "wishing" that P.V. would throw the right faces. P.V. wished, in not observing alternation of targets, to prevent C.V. from influencing the experiment. He therefore wrote his choice of target on a slip of paper which he put out of sight until the throwing was completed, whereupon he showed it to C.V., who recorded it. Since P.V.'s results were not different from chance, however, it is doubtful that this part of the procedure made any difference.

At the conclusion of the experiment we analyzed the data for PK effects reported in earlier research by other experimenters and then sent the original records to the Parapsychology Laboratory, where they were rechecked.

Results

General Results

The general results, shown in Table 1, are as follows: On high dice C.V. threw 135 runs with a deviation of +69 (CR = 3.48, P = .0005). For low dice she threw 111 runs with a deviation of +45

(CR = 2.50, P = .012). Thus C.V.'s scores were independently significant in both categories. Her total for all dice was 246 runs with a deviation of +114 and a CR of 4.26 (P = .00002).

Table 1

RESULTS OF HIGH-DICE AND LOW-DICE SERIES

Subject	HIGH-DICE				LOW-DICE				TOTAL			
	Runs	Dev.	CR	P	Runs	Dev.	CR	P	Runs	Dev.	CR	P
C. V.....	135	+69	3.48	.0005	111	+45	2.50	.012	246	+114	4.26	.00002
P. V.....	96	+11	.66	.5	138	+10	.50	>.5	234	+21	.80	.4
Total...	231	+80	3.08	.002	249	+55	2.04	.04	480	+135	3.61	.0003

There was a greater discrepancy in P.V.'s case between the number of throws for high and low dice: 96 runs of the former and 138 runs of the latter. Neither was significant. The 96 runs of high dice gave a deviation of +11 and the 138 runs of low dice, a deviation of +10. The total score of 234 runs with a deviation of +21 is insignificant.

When the work of both subjects is combined the high dice are still significant. The 231 runs, with a deviation of +80, have a CR of 3.08. The 249 runs of low dice have a deviation of +55 with a CR of 2.04. The grand total for both subjects under both conditions is also highly significant: 480 runs, with a deviation of +135 and a CR of 3.61.

Distribution of Hits Within the Two Sets on the Page

The distribution of hits on the page shows pronounced differences in the rate of scoring of the two subjects. Each record page was composed of two columns or sets, one on the left-hand side of the page and one on the right, with a central space in between where the target selections and final scores were noted. Each of these columns from top to bottom contained three runs. When the page was analyzed for scoring rates in these left-hand and right-hand *sets* for both subjects, it was found that C.V. showed a notable decline which was especially marked for the left-hand set on the page. (See Table 2.) Her highest average score was made on the first run and her lowest one on the middle run of the set. There was, however,

only a slight rise in success on the third run, though this is noteworthy as suggesting an ending salience such as has been reported for a number of other subjects. For the pooled set of all her data the first run score is significantly higher than the second (CR_d = 3.38), and the difference between the first and third is also significant (CR_d = 3.11).

Table 2

VARIATION OF AVERAGE RUN SCORES WITHIN THE THREE-RUN SETS

Subject	Run Within Set	LEFT-HAND SET		RIGHT-HAND SET		ALL SETS POOLED	
		High Dice	Low Dice	High Dice	Low Dice	Average Score	CR_d
P.V.		(18)*	(21)*	(14)*	(25)*	(78)*	
	1	4.72	4.76	4.71	4.92	4.79	(1−2)= −1.22
	2	4.83	5.14	5.21	5.28	5.13	(1−3)= −2.02
	3	5.50	5.71	5.79	4.68	5.35	
C. V.		(25)*	(16)*	(20)*	(21)*	(82)*	
	1	6.20	6.56	5.85	5.67	6.04	(1−2)=3.38
	2	5.16	5.00	5.10	5.24	5.13	(1−3)=3.11
	3	5.12	5.13	5.65	4.95	5.21	

*The figures in parentheses show the number of sets in each category or the number of runs upon which each group of three averages is based.

P.V.'s distribution in the pooled set for his total data, however, shows a consistent though statistically insignificant incline in scoring within the set. The averages for the three runs in the set were 4.79, 5.13, and 5.35, respectively. The variation in scoring within the three-run set for both subjects is graphically illustrated in Figure 1.

Further analyses showed no significant scoring effects within the individual runs nor in the chronological distribution of scores throughout the experiment.

DISCUSSION

In view of the fact that the results of both subjects pooled give a significant total deviation, we may discuss the results as explainable on some basis other than pure chance. We may reasonably suppose, too, that the method of throwing the dice eliminates the possibility of controlling them by the unconscious use of skills. If our recording is subject to any reservation on the point of careless-

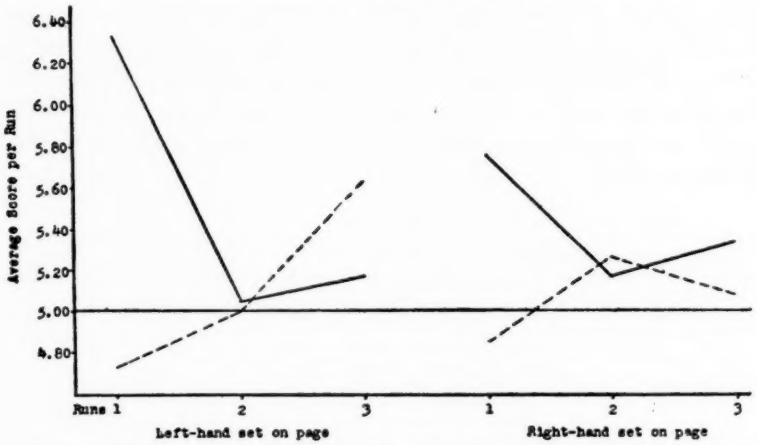

FIG. 1. Variation in scoring rate within the three-run sets for subject C.V. (solid line) and subject P.V. (broken line), high dice and low dice combined.

ness or wishful thinking, it is at least worth noting that the case rests primarily on P.V.'s recording, and his professional training and experience should at least have prepared him for a reasonable order of accuracy in such matters. That is as far as the question can be settled.

In the way of secondary assurances or confirmations we may point to the fact that after the series of tests was completed and the distribution of hits examined, it was found that in C.V.'s section of the results there was a very significant decline from the first to the second run on the page. This internal difference is in the direction previously found by Reeves as well as by other investigators.

If we are justified in concluding that PK operated in the production of the results, there is little doubt that it was C.V. alone who was responsible for the observed statistical significance. The evidence shows not only that C.V. produced almost all of the positive deviation but also that she showed pronounced scoring trends within the set. Without basing any conclusion on the comparison, it is interesting, however, to observe that C.V. and P.V. followed opposite trends in their scoring through the three runs of the set. Whereas on both the left-hand and the right-hand set C.V. began high on the first run and dropped considerably on the second run, rising a little

to the third, P.V. began at his lowest on the first run, rose considerably on the second, and then got his highest average on the third run. His trend is an incline, whereas C.V.'s is a decline.

This difference between us shows clearly enough that these distribution patterns which are now very conspicuous in parapsychology reports are to some extent individual. If the individual subjects have different attitude reactions to the test and show in general a different psychological development in the course of the test, we must expect these differences in trend. It is particularly important to bear this in mind in comparing our results with those of Reeves. While our decline in the set is similar, we did not obtain the very marked position effects in the run of twelve trials which characterized the Reeves performance.

Is the factor brought out by these dice-test results the same one which produced the effects in the seed tests and is it the potential cause of the physical disturbances in our household which initiated these inquiries? We cannot answer these questions with finality. The difference in scoring rates obtained by the two of us suggests an affirmative answer, and that is at least encouraging, not only for the continuance of the PK studies themselves but for the application of this approach to other cases in which unexplained physical disturbances appear and do not lend themselves to study by other methods. Perhaps this we may claim as our contribution—this illustration of at least one way of working out from a puzzling problem to the beginning of a scientific answer. It is perhaps noteworthy, too, that through the period of high concentration on the dice-throwing experiments which were crowded into the limited leisure time afforded by a busy professional life, there were scarcely any recurrences of the spontaneous physical effects. One can only ask whether there was a complete utilization of some reserve force that otherwise might have discharged into spontaneous phenomena if allowed to accumulate or, again, whether the more systematic direction of the interest of the family into an absorbing, constructive research would not better account for the fact. The research leaves many such questions unanswered.

REFERENCES

1. MONTANDON, R. *Les Radiations Humaines.* Paris: Lib. Felix Alcan, 1927.
2. REEVES, M. P., AND RHINE, J. B. The PK effect: II. A study in declines. *J. Parapsychol.,* 1943, **7,** 76-93.
3. VASSE, P., AND VASSE, C. Influence de la pensée sur la croissance des plantes. *Revue Métapsychique,* No. 2, 1948, 87-94.

136 Boulevard de Chateaudun
Amiens
Somme, France

AN EXPERIMENTAL STUDY OF ESP CAPACITY IN MENTAL PATIENTS

By K. E. Bates and Marietta Newton

ABSTRACT: In 1938 fifty patients at the Ohio State Hospital were given ESP card tests by Miss Margaret Price who was at that time a research assistant in the Parapsychology Laboratory. These patients represented nine different classifications of mental illness. Altogether, the 50 subjects participated in 1700 ESP card runs in which they were asked to get as many hits as possible (high aim) and 495 runs in which they were asked to get as few hits as possible (low aim). In both categories the scores were highly significant.

When the scores were examined according to whether the subjects were classified as cooperative, apathetic, or irritable, it appeared that the cooperative subjects were the most successful (that is, they scored highest in high aim and lowest in low aim); the apathetic subjects were next; and the irritable subjects were the least successful. In the high-aim results, the difference between the cooperative and the irritable subjects was statistically significant. The authors conclude that subjects' attitudes are an important factor in their scoring.

No inherent differences in scoring ability for the different mental disorders were shown.

The authors, Mr. Bates and Miss Newton, were graduate research assistants in the Parapsychology Laboratory at the time this paper was written. They prepared this article on the basis of the original data on file in the Laboratory, since the experimenter was unable to do it herself.—Ed.

THIS REPORT is a review of an ESP experiment in which the subjects were mental patients at the Ohio State Hospital in Columbus, Ohio. The work was carried out by Miss Margaret Price in 1938, and although referred to in the literature (4), it has never been fully reported. The surplus of experiments which demanded immediate attention at that time prohibited the publication of much of the interesting material then available. Since there was no second observer present in this experiment during the testing, the work did not meet the more stringent experimental requirements of that time, and it had little chance for immediate publication. However, in view of the current inquiries into the correlation of personality factors with the scoring levels obtained by ESP subjects, this work should have considerable interest.

The experiment offers the opportunity to compare the results with those from other researches along similar lines. One of these researches was carried out by Robert Shulman who, in 1937, con-

ducted some card experiments with "psychopathic" subjects at the Hudson River State Hospital in Poughkeepsie, N. Y. Others were researches relating to the attitude of the subjects. In Miss Price's research the 50 subjects[1] were all rated according to their attitude of general cooperativeness. This study of subject attitudes is quite apropos of work done by other investigators indicating that ESP performance is related to personality attributes. For example, the report by Humphrey in the present number shows that extraverted persons (as measured by the Bernreuter Personality Inventory) scored higher than introverts. Also, attitudes of *belief* about the ESP test itself have been found to be related to ESP performance in the work of Schmeidler (**5, 6**), Bevan (**1**), Casper (**2**), Kahn (**3**), and others; and, although predating the current research by more than ten years, this work of Price might be considered in some measure a confirmation of these later findings.

A considerable amount of evidence collected in this study cannot be reported. Miss Price kept notes on her clinical impressions of the patients in the experimental situation. Her notes include data on the educational background of the subjects, their interests, ages, and sex, as well as changes in performance from day to day. Much of the material is too general and informal to be treated here and, consequently, our attention will be focused on a few factors which were consistent throughout the work and which received the greatest experimental emphasis.

Procedure

Three test procedures were used in the experiment: the STM or screened-touch-matching technique in which the subject indicated through a one-way screen his choice of the five targets which he thought would match the card held by the experimenter on the other side of the screen; the psychic shuffle in which the subject attempted to shuffle one ESP deck to match the order of another screened target deck; and the BM or blind matching technique which was the most popular of the procedures since it did not necessitate the use of a screen and since it permitted the subjects to see the experimenter. In this technique the five target cards were concealed with

[1] A review of these findings in *Extrasensory Perception After Sixty Years* (**4**) reports 49 subjects instead of 50. This and other discrepancies are due to the fact that one subject was inadvertently omitted from that report.

cover cards and the subject sorted the target deck into five piles to correspond with the targets. When a run (one sorting of the standard 25-card deck) was completed, Miss Price exposed the target cards and asked the subject to sort out the correct cards in each of the five piles. The subject counted the number of hits in each pile, passing the incorrect cards to Miss Price. When the recording had been finished, Miss Price recounted each pile of correctly matched cards as a double check on the recording.

The subjects were sometimes asked to try for low scores instead of high scores. A total of 1,700 runs were done for high aim by all 50 subjects. Four hundred and ninety-five runs were accomplished as low-aim tasks by 45 of these subjects. All subjects were clinically classified in two ways: first they were divided according to mental disorders as diagnosed by the hospital staff; and second, they were classified as either cooperative, apathetic, or irritable. This second classification was made on the basis of ward reports by the experimenter together with the doctor or the ward head. In case of disagreement, the final decision regarding classification of a patient was reached by consultation. The experimenter was not able to rate all the subjects before they took part in the experiment, but she based her judgment upon hospital records that identified the patients only by number. Since she did not associate the number with the name until she had classified the subject as cooperative, apathetic, or irritable, and since the doctor and ward heads did not know the ESP scores in any instance, the ratings may be considered as having been made independently of any knowledge of the ESP scores.

Results

Seventeen hundred runs were carried out by 50 different subjects, who were instructed to get as many hits as possible (high aim). They scored a positive deviation of 911 hits above chance expectation, with the highly significant critical ratio of 11.05. Forty-five of the subjects were asked to get as few hits as possible in the tests (low aim). Their 495 runs done under this condition were also successful, the negative deviation of 173 hits below chance expectation giving a critical ratio of 3.89.

Table I gives the results for the total experiment broken down into high- and low-aim tasks as well as test procedures. The

probability that the high-aim scores alone would have achieved the high positive deviation reported is 10^{-28}; and although less spectacular, the low-aim category is also independently significant, with a probability of .0001 for the negative deviation obtained. A test of the difference between high- and low-aim data gives the highly significant probability figure of 10^{-17}.

Table 1

GENERAL RESULTS

Number of Subjects	Condition	Runs	Deviation	Average Run Score	CR	P
47.....	High-aim BM	941	+535	5.57	8.72	10^{-17}
16.....	High-aim STM	213	+ 72	5.34	2.47	.014
44.....	High-aim PS	546	+304	5.56	6.50	10^{-10}
50.....	Total High Aim	1700	+911	5.54	11.05	10^{-28}
42.....	Low-aim BM	460	−158	4.66	3.68	.0002
4.....	Low-aim STM	35	− 15	4.57	1.27	.2
45.....	Total Low Aim	495	−173	4.65	3.89	.0001

A further breakdown of the results is furnished by Table 2, where average scoring levels for high- and low-aim tests are shown in their relation to nine specific mental disorders. Scores for six of the nine categories in the high-aim section and two of the nine in the low-aim section were individually significant, but the nine types of illness did not vary one from another to any significant degree.

An analysis of the experimental results from the point of view of subject attitudes is supplied by Table 3. The high- and low-aim classifications have been maintained so that these results may be compared with those of Table 1. Note the consistency within Table 3 for high- and low-aim conditions: the best results in the case of high aim were obtained from cooperative subjects both in terms of average score and CR. The next most successful group on high aim, the apathetic subjects, were better than the irritable subjects both in average score and CR. In the case of low aim, these same attitudes produced consistently similar results. Thus, throughout the experiment the cooperative subjects were able to score higher on high aim or lower on low aim than apathetic or irritable subjects. The irritable subjects, although significant on high aim, were never-

Table 2

ESP Results in Relation to Mental Disorder

Diagnosis	High Aim				Low Aim			
	Number of Subjects	Runs	Average Run Score	CR	Number of Subjects	Runs	Average Run Score	CR
Involutional	6	194	5.49	3.41	4	45	4.18	2.76
Hebephrenia Dementia Praecox	7	240	5.62	4.81	7	90	4.84	0.74
Catatonic Dementia Praecox	6	201	5.47	3.35	6	70	4.73	1.14
Paranoid Dementia Praecox	10	322	5.44	3.98	10	100	5.02	0.10
Manic-Depressive, Manic	8	265	5.55	4.51	7	70	4.24	3.17
Manic-Depressive, Depressed	5	200	5.76	5.37	3	30	4.60	1.10
Neurasthenia	2	65	5.54	2.17	2	20	3.95	2.35
Misc. Organic	5	175	5.37	2.46	5	60	4.77	0.90
Behavior Disorder	1	38	5.79	2.43	1	10	4.50	0.79

Table 3

ESP Results in Relation to the Cooperativeness of Subjects

Attitudes	High Aim				Low Aim			
	Runs	Dev.	Average	CR	Runs	Dev.	Average	CR
Cooperative	715	+510	5.71	9.54	215	−111	4.48	3.79
Apathetic	530	+258	5.49	5.60	160	− 62	4.61	2.45
Irritable or Non-cooperative	455	+145	5.32	3.40	120	− 10	4.92	0.46

CR_d (Cooperative-Apathetic): 1.93 (High Aim) 0.62 (Low Aim).
CR_d (Cooperative-Irritable): 3.25 (High Aim) 1.93 (Low Aim).

theless least successful on low aim. A test of significance between high-aim cooperative and high-aim irritable categories gave a CR of 3.25; no significant differences were found in cooperativeness for low-aim conditions.

Although the detailed results are not included in this report, a further analysis was made of the relation of ESP success to both cooperativeness and the disorder groupings shown in Table 3. It was found that the cooperative subjects gave more significant ESP scores regardless of the diagnosis of their mental disorder. In other words, the effect of attitude was independent of the type of mental illness.

Discussion

If inherent differences in scoring ability obtain for different mental disorders, they would not seem to have been unmasked by this experiment. Shulman found his greatest differences to exist between the positively scoring manic-depressive depressed patients and the negatively scoring involutional melancholia patients. Miss Price obtained high scores with both groups. It is worth noting that every group with the exception of paranoid dementia praecox succeeded in obtaining deviations in the desired direction. The probability for the high-aim and for the low-aim conditions and the probability for the differences between the two require some other factor than chance to explain the results.

The significant difference between cooperative and irritable subjects confirms experimental evidence that subjects' attitudes are an important factor in ESP experiments. It will be recalled that another measure of attitudes, the sheep-goat differentiation (which deals with the subject's attitude toward ESP and toward his own ability to succeed in an ESP test), has proven to be one of the most successful correlates to ESP performance. Also, the separation of subjects on the basis of introversion and extraversion, which may be related to the degrees of cooperativeness in the present study, gave the most striking separation of high and low scores yet obtained.

There is one important difference between this study of subjects' attitudes and those of other investigators. All of the groups in this experiment obtained deviations in the direction of their aim, scoring positively on high aim and negatively on low aim. In other studies, however, the separation of subjects almost always divided positively scoring subjects from those who scored negatively. It would be worth while to make a further study of the relation of general cooperativeness to ESP performance in normal subjects if a basis of classifying the subjects in this regard could be found. It is an interesting question whether the distinctions drawn in the present experiment were not peculiar to institutionalized psychopathic subjects.

A question which may be even more significant and probably more vital to an interpretation of the results is that of *how* such significant scoring was obtained in the first place. A great deal may depend upon the ability of the experimenter to understand the

subject and skillfully assist in releasing the inhibitions which are often so fatal to successful ESP experiments. In this respect Margaret Price certainly excelled. While studies continue on the personality factors of successful subjects, someone might profitably introduce one on successful experimenters.

References

1. Bevan, J. M. The relation of attitude to success in ESP scoring. *J. Parapsychol.*, 1947, **11**, 296-309.
2. Casper, G. W. A further study of the relation of attitude to success in ESP scoring. *J. Parapsychol.*, 1951, **15**, 139-45.
3. Kahn, D. Studies in extrasensory perception: experiments using an electronic scoring device. (Unpublished ms.)
4. Rhine, J. B., *et al. Extrasensory Perception After Sixty Years.* New York: Henry Holt, 1940.
5. Schmeidler, G. R. Separating the sheep from the goats. *J. Amer. Soc. psych. Res.*, 1945, **39**, 47-49.
6. Schmeidler, G. R., and Murphy, G. The influence of belief and disbelief in ESP upon individual scoring levels. *J. exp. Psychol.*, 1946, **36**, 271-76.

Parapsychology Laboratory
Duke University
Durham, North Carolina

NOTES AND COMMENTS

REFLECTIONS ON PARAPSYCHOLOGY, PSYCHOANALYSIS, AND ATOMIC PHYSICS

By Pascual Jordan[1]

Under the title "Verdrängung und Komplementarität" (Repression and Complementarity) I published in 1947 a short monograph[2] developing theoretical aspects of psi phenomena in relation to Freudian psychoanalysis and to modern atomic physics. Indeed, there are relations—and very interesting ones.

Some of these may be discussed without reference to psi phenomena. The leading conceptions of the psychology of the unconscious mind and of quantum mechanics show some common features which have not been analyzed hitherto. Let us first examine the central phenomenon of psychoanalysis—we prefer a description that is perhaps a little unorthodox. Suppose there is a man who decides to go on a trip. He goes to the station, but several lapses and blunders on his part make it impossible for him to get his train. Afterwards he goes to a psychoanalyst who makes him feel and perceive that in his unconscious mind he had some intentions which were in contradiction with his decision to travel. Only by perceiving clearly these unconscious intentions are they dissolved and cease to exist.

We have here a situation fulfilling exactly all requirements of the conception of complementarity as developed by quantum mechanics. This concept may be explained as follows. We have here an electron, and we are able to make an exact observation of its impulse or wavelength. But then this same electron does not possess any definite location. Now we can make an observation of the place of this electron and observe the result. But in this case the wave length (the impulse) of this electron becomes totally undefined. We cannot make both observations simultaneously; in the attempt to make accurate measurement of the value of one of these quanti-

[1] Prof. Jordan is a German physicist of international fame, a specialist in quantum mechanics and biophysics. He is at present a visiting professor at the University of Hamburg.—Ed.

[2] Hamburg-Bergedorf, 1947 (Strom-Verlag).

ties at a given instant, the condition of the electron is altered in such a way as to render impossible a precise simultaneous measurement of the other quantity. We cannot make observations without changing at the same time the state of the object.

The relation of the conscious and unconscious mind, as revealed by psychoanalysis, shows close and deep analogies to this quantum mechanical situation. We do not intend to proceed here to a more thorough examination of these analogies—as given in my book—but let us take note of some important points. It is a fundamental fact of Freudian therapy that the state of the unconscious mind cannot be "observed" without changing it. The dissolution of unconscious complexes by making them conscious is another case of what we physicists call "complementarity."

The man cited above, running to the station but forgetting several necessary things for his journey because unconscious intentions or complexes interfered with his conscious decisions, is only a less pronounced case of those phenomena of split personality often referred to in the literature of abnormal psychology. A typical case would be as follows: There is a man who sometimes feels and declares himself to be Mr. A; but on another day he says, "I am Mr. B." Mr. A and Mr. B are two widely different persons, with their own memories and their own desires and intentions; and each one belongs totally to the unconscious part of the other personality. In order to observe Mr. A we have to make Mr. B "unobservable," perhaps by hypnotic suggestion, for we can never observe Mr. A and Mr. B at the same time. Here is a situation analogous to the fact that we cannot observe an electron at the same time as Mr. Wave and Mr. Particle.

Closely related to such applications of the chief conceptions of modern quantum mechanics to the phenomena of the unconscious mind—indicating wide unexplored fields of future research—I discussed in my book the consequences of a radical application of the fundamental aspects of physical "positivism" to psychology and parapsychology. The chief point is that we must abandon the traditional conception of metaphysical reality, as existing independently from any conscious or unconcious mind.

According to current, traditional ideas there exists my conscious mind, and in addition to this there exists my unconscious psychic

realm. The main part of the former consists of normal perceptions, and we are accustomed to regard these perceptions as perceptions of an "outer world" of reality. They are in close and rigorous relation to those of other individuals; we may say they have a social function. But we generally believe the unconscious realm of each individual to be of a strictly individual and private character. Dreaming of a table, I do not expect other persons to dream about the same table; but perceiving a table, I think it natural that other persons too are able to perceive it.

The analysis of this situation is strongly hampered by the traditional idea of "reality" as existing independently of any human mind. We say that we perceive real objects and that there is a fundamental metaphysical difference between the table perceived and acknowledged as reality and the table only dreamed of. But why? If we discard traditional ideas, there remains only the difference based on social as against the individual meaning; and this favors the suspicion that the distinctive difference indeed may not be absolute but one of degree only.

Let us assume that to a certain extent my dream about a table of a certain shape influences other persons to dream of it also. To conjecture that this might be the case is a very natural hypothesis if one assumes the transition from conscious to unconscious experience to be a matter of degree only. What would we think about such a phenomenon? We would call it "telepathy"; it is the type of phenomenon represented by the empirically established facts of parapsychology.

I shall not try to go more deeply here into the matter—it is discussed further in my book. The existence of psi phenomena, often reported by former authors, has been established with all the exactness of modern science by Dr. Rhine and his collaborators, and nobody can any longer deny the necessity for taking the problem seriously and discussing it thoroughly in relation to its connections with other known facts. Having just read Dr. Rhine's exciting book, *The Reach of the Mind,* I see that he denies strongly any possibility of "explanation" of psi phenomena in terms of physics; and this indeed is exactly also my own opinion, as developed in my monograph.

Let us abandon the traditional idea of reality. The true meaning

of reality lies in the social significance of normal perception, in the community of different individuals founded upon the fact that one's own perception of a table enables him to say that other individuals also will be able to see it. Thus we are induced to conjecture that my unconscious mind has similar relations to the unconscious mind of other persons, that the traditionally supposed strict individuality of unconscious experiences does not exist, that the unconscious experiences of different individuals show similar relations to those of their normal, conscious perceptions. Then we gain a picture of Nature and Mind which is wide enough to include the empirical facts of parapsychology. And it becomes clear that it is not possible to "explain" the facts in terms of physics, for the physical world, from this viewpoint, is only a bordering zone of the world as a whole (differentiated by relations of complementarity) just as our conscious mind is only a bordering surface of the deep ocean of our whole psychic life.

BOOK REVIEWS

THE CLAIRVOYANT THEORY OF PERCEPTION. By M. M. Moncrieff. Foreword by Prof. H. H. Price. London: Faber and Faber, 1951. Pp. 315.

The author of this book is familiar with the contemporary theories of perception, as well as with the available records of facts concerning clairvoyance. Taking it as established that the latter is a genuine faculty, he declares: "The thesis we put forward is that in *Ordinary Vision,* when we are said to perceive (visually) an 'external physical object' we do *not* perceive sensa or sense-data or sensible appearances—in some mysterious way associated with that object—but we *directly* perceive that thing, or *directly cognize* that thing. In brief, we would contend that in ordinary vision we have 'clairvoyant perception' instead of 'sensory perception' " (p. 22). He further contends that, in ordinary visual perception, clairvoyance is channelled by the visual organs and thereby limited to such facts as are biologically important to the organism; that the perceived surface characters of material objects really exist there independently of their being perceived; and hence that the view of perception known as Naïve Realism is correct. All this, the author declares, is based on the "Principle of Four-dimensionality."

The idea that ordinary visual perception may be a special case of clairvoyance is of course not original with the author, but, as Professor Price remarks in his foreword, Mr. Moncrieff is the first to have attempted to work it out in detail.[1] For reasons to be stated

[1] The idea has been put forward, for example, by Thouless and Wiesner in an article, "The Psi Processes in Normal and Paranormal Psychology," *Proc. Soc. psych. Res.*, Vol. XLVIII, Dec. 1947, p. 180: ". . . in normal thinking and perceiving I am in the same sort of relation to what is going on in the sensory part of my brain and nervous system as that of the successful clairvoyant to some external event, and . . . this relation is established by the same means"; to wit, by ESP. Professor J. B. Rhine, we believe, also has made somewhere a similar suggestion; and the idea probably has been advanced by still other writers. The present reviewer himself, indeed, also has offered it: ". . . sensation could be viewed as a special instance of clairvoyance, and the sense organs, together with the corresponding nerves and brain regions, as specialized transmitters, which, like microscopes, magnify what they transmit, but transmit at all only what occurs at relatively short distances and in circumstances of special kinds" (pp. 18, 19 of *Is a Life after Death Possible?* the Foerster Lecture on the Immortality of the Soul, University of California, May, 1947; also, pp. 427-28 of *Nature, Mind and Death*, Open Court Publishing Co., 1951.)

The suggestion of Thouless and Wiesner, like this one, is that in vision, the

in the sequel, it seems to this reviewer that in spite of its length, documentation, and indefatigable ingenuity, the book wholly fails to support its contentions and adds nothing to present understanding of either perception or clairvoyance. Rather, the haziness of its key concepts and the gratuitousness of some of its tacit assumptions tend to obscure the real issues. To comment on all the author's criticisms of current theories of perception, and on all of his own many proposals, would require a whole book. We shall, therefore, have to confine ourselves here to examination of the key concepts of his theory.

Let us note first how Mr. Moncrieff defines clairvoyance. It is, he writes, "Perceptual Awareness of External Material Things derived otherwise than through the medium of the normal visual Sensory Organs" (p. 21). He distinguishes several species of it; especially (1) "Travelling Clairvoyance," in which visual perception is from a point of view which may be at any distance or locality outside the physical body of the percipient (p. 38); (2) "Four-dimensional Vision," i.e., "perception of an external material thing from a number of simultaneous 'points of view' outside it," or even, in addition, inside it (p. 39); (3) "Common Perceptual Clairvoyance," which "is dependent on stimulation of the retina by incident electro-magnetic radiations," and "its field of vision . . . limited and affected by the physiological structures in front of the retina" (p. 42). It is this third species of Clairvoyance which the author believes constitutes ordinary visual perception.

Several comments now suggest themselves. One is that the *dependence,* which the author here asserts, of this third form of clairvoyance on the retina and other parts of the visual sense organs plainly contradicts his general definition of clairvoyance as perception "otherwise than through the medium of the normal visual sensory organs." To say, as he does, that the role of the cortical events induced by retinal stimulation is to "evoke" or "release" clairvoyance of course does not avoid the contradiction; nor do these verbs—which, no less than "to cause," are verbs of causality—possess any

physical events in the occipital region directly cause psychical events; to wit, visual sensations; and that direct physico-psychical causation is precisely what clairvoyance consists in—the only difference being that in the latter the directly causative physical events are external, not somatic ones. Mr. Moncrieff's contention, as will be seen, is different from this.

magic, absent from the latter verb, that would entail that what is "evoked" or "released" is not, as commonly would be said, *sensations,* but instead, as the author asserts, clairvoyance.

Moreover, although Mr. Moncrieff speaks again and again of "direct perception," he never states what kind of process exactly he means by these words. He contents himself instead with italicizing the word "direct" almost every time he uses it. But, without an account of what "direct perception" means, one cannot tell whether or not clairvoyance is an instance of it, nor therefore whether or not ordinary visual perception, even if it is clairvoyant, is "direct." That clairvoyance, in some of its forms, occurs without the intermediary of *the eye* leaves quite open the possibility that, in clairvoyance, what is *directly* present to consciousness is *images of things,* not material things themselves—those images being generated or evoked in the mind through a causal process of some paranormal kind, instead of, as in ordinary perception, by cortical events resulting from stimulation of the eye.

Incidentally, although the author speaks innumerable times of material objects as perceived 'out there' (always putting these words between inverted commas), it never seems to occur to him that "out-there-ness," and for example, "blueness," have precisely the same status in visual perception, viz., that of being "seen," no matter whether that status turn out to be subjective, or, in some sense, objective. Nor does the author ever appear to realize that—except for the radical sort of materialism which, by "mind," means "brain" —externality to the body is one thing, and externality to the mind quite another thing, and the latter not definable in terms of locations in perceived space.

The only passages in the book that might appear to provide a meaning for the terms "direct perception" or "immediate perception" are those (e.g., p. 23) where the author states that by them he means *compresence*—a term which he borrows from S. Alexander but uses in a modified and even vaguer sense. For Alexander, a mind consists of a continuum of certain neural processes which, because they "have the conscious quality" are identical with "acts of mind." These "enjoy" (i.e., intuit) themselves, and "contemplate" (i.e., perceive) their "objects" (e.g., sensa). As to "compresence," Alexander explains that if B is a patch of red and A a

mind, and "*B* acts causally on the body of *A* and excites a mental process, *a*," of seeing, then "the two compresents are *B* and the mental process *a*." Cognitive compresence for him thus does not mean simply coexistence, nor even simply existence together in space-time of an "object" and a mind (that is, a "conscious" brain); but, in addition, that the "object" operates causally on the sense organs directly and, through them, excites the "cognitive" neural processes.[2]

Now, it is evident that this amounts only to saying that compresence of an "object" and of an act of mind excited by it in the manner just described is *cognitive* compresence because acts of mind are "cognitive" of the objects that excite them! Thus, what "cognitiveness of" (or "consciousness of" or "contemplation of") *itself* consists in is not really analyzed at all by Alexander. His analysis is only of what *leads up to* the "cognitive act."

The modification Mr. Moncrieff would make in Alexander's notion of cognitive compresence would be to say that cognition of a physical thing is brought about (not as Alexander says "through the direct operation of the thing upon the senses," but) through the direct operation of the thing "upon the mind *by the Faculty of Clairvoyance*." But obviously, if this supposition is to explain anything, that faculty—which is here putatively a somehow "cognitive" faculty—is just as much in need of analysis as is Alexander's "contemplative cognition"; but the analysis is not provided, nor does Mr. Moncrieff even define, whereas Alexander does, what he means by "a mind."

One might expect that since Mr. Moncrieff's theory of visual perception is said by him to be "ultimately based on the 'Principle of Four-dimensionality,'" the chapter he gives to this would provide the needed analysis. Unfortunately, his attempt to say what this principle is—an attempt which he himself regards as not particularly successful—is likely to leave his reader with the impression that "The Principle of Four-dimensionality" is nothing but the name of a *deus ex machina* which is invoked—like "God's agency" by the pious—whenever something needs to be explained which he cannot explain. It explains anything simply because it is postulated as having the capacity to do so!

[2] S. Alexander, *Space, Time and Deity*, London: Macmillan, 1920. Vol. I, pp. 11 ff.; Vol. II, pp. 81, 82 ff.

Of course, the supposition that space has a fourth dimension has been appealed to with some measure of success by various writers to explain how certain paranormal phenomena are possible.[3] But Mr. Moncrieff is emphatic that "Our notion of 'Three-dimensionality' and 'Four-dimensionality' . . . is entirely different from the terms three-dimensional and four-dimensional, which latter are only applicable to the various kinds of three-dimensional and four-dimensional worlds, whether purely spatial or spatio-temporal or mathematical. . . . 'Four-dimensionality' is not a term indicative of a 'dimension' additional to a 'third dimension' . . . but . . . of a state of existence in which the spatio-temporal characteristics of lower dimensional worlds are integrated into either simultaneous *wholes* or indivisible *qualitative wholes*" (p. 65). This is as near as the author ever gets to saying what that magical principle is, for nowhere in the chapter does one find a forthright definition of it.

The last words of that quotation allude to Mr. Moncrieff's assumption that lower orders of reality are merely "quantitative," but higher ones "qualitative"—as if there could be such a thing as a quantity which is not of some "what," or a quality that does not have some quantity! He asks how the lower orders are integrated into the higher and answers that "the operating factor which performs this function is the integrating characteristics of the Principle of Four-Dimensionality" (p. 67).

To illustrate what he means by "integration," the author mentions that, according to the physicists, the color red is "associated with 400 billion electro-magnetic frequencies per second. The latter from the standpoint of this Principle (of Four-dimensionality) represent the characteristics of 'Three-dimensionality' since they are successive and quantitative in nature and constitute aggregates. On the other hand, the colour, red, represents the characteristic of 'Four-dimensionality,' since it is an indivisible qualitative thing and represents an integral whole" (pp. 67-68). Mr. Moncrieff then asserts that "when we perceive the colour red, we grasp together or prehend simultaneously, that is, as one indivisible qualitative whole, 400 billion successive physical events in one second . . . each frequency or wave-motion being regarded as a physical event" (pp. 68-69).

But obviously the alleged "prehension" of these 400 billion physi-

[3] Zollner, Lombroso, Whately Smith (Carington), C. H. Hinton, and others.

cal events is pure myth. The author's need to postulate it arises only from his tacit but erroneous assumption that the cause of an event must somehow be hiddenly present in its effect—an assumption closely similar to that which psychologists have termed "stimulus error." At various places in other chapters, the author's remarks postulate tacitly, and equally gratuitously, that the effect of a cause must somehow resemble—e.g., be isomorphic with—that cause.

Bearing in mind that the "Principle of Four-dimensionality" is nowhere formulated more explicitly than in the passage quoted, we may cite, as samples of the logic in the chapter devoted to it, one or two additional passages as follows: "Since the colour quality, red, and light, derive their existence from an *objective*-integrating principle, namely, the Principle of Four-dimensionality, both are *objective* existents 'out there' and are *not* dependent on the mind-body organism of a perceiver" (p. 68). Again: it is "in virtue of our minds possessing the characteristics of 'Four-dimensionality,' or in brief, of our possessing 'Four-dimensional consciousness,' " that we are able to do such things as perceiving the beauty of a sonata or a sunset, or recalling past experiences by memory, or grasping "the 'inner' nature and significance of the outward appearances of things" (p. 70).

To do full justice to the logic of the book, however, lengthier and more numerous quotations would be necessary. The reviewer believes that they would further vindicate the conclusion that the author wholly fails to make out a case for what he calls the "clairvoyant theory of perception."

C. J. Ducasse.

Prof. Ducasse is a member of the Philosophy Department of Brown University.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

visions? The point is well taken; we doubt very much that the procedures of scientific investigation of psychic powers will inspire anyone to very much. But the birth of an idea that *is* inspiring may at least be eased and made more possible through the conscientious efforts of men who are attempting to wear away the skepticism that has been slowly built up over two or three hundred years.

. . . . . .

It is not rigid skepticism which preserves the integrity of the scientific spirit, but disciplined open-mindedness and an unending quest for profounder relevances than have heretofore been recognized. It seems likely—at any rate, we are convinced that it is so—that a general reconsideration of the psychic factor in human life will be necessary before there can be a fruitful psychology and sociology, and before, indeed, there can be a social movement with genuine promise of the betterment of the world we live in. Man, to better himself, must first learn to honor himself, and while the realities of psychic phenomena are not necessarily uplifting in character, they often suggest the presence of hidden potentialities in human beings. They may, therefore, contribute to a conception of man that will move the heart and mind of man to greater things.

From "Psychic Possibilities," *Manas*,
October 17, 1951

IMPORTANCE OF THE PSYCHIC FACTOR IN HUMAN LIFE

There is the question, first, of the nature of the world around us. How might our conception of the world of nature be altered by acceptance of the reality of the psychic factor?

. . . . . .

If there is anything at all conclusive about the findings of modern psychology, including both academic research and the clinical investigations of analysts and psychiatrists, it is that man's *idea of himself* is of supreme importance in its effect upon moral decision and behavior generally. Citations from scientific literature, although they exist in abundance, are hardly needed to show that the energies and capacities of a human being increase or decrease in direct relation to what he thinks of himself, his past, present, and future. . . .

. . . . . .

Have we but lately oozed, then clambered, up from the primordial slime, and is our dream of progress but the unrolling of the promise of "biological" potentiality? Must we, as Thomas Huxley insisted, learn to oppose the "cosmic process" of dog eat dog, and abandon the heritage of an ancient tribe of beetle-browed ape-men whose jungle ways are still theorized upon as the limiting foundation of subsequent human relations? Or have we a dual heritage—one coming from beyond as well as one coming from below? Wordsworth has put it well:

> Our birth is but a sleep and a forgetting;
> The Soul that rises with us, our life's Star,
> Hath had elsewhere its setting
> And cometh from afar;
> Not in entire forgetfulness,
> And not in utter nakedness,
> But trailing clouds of glory. . . .

But, someone well may ask, can the humdrum routine of guessing at ESP cards lying face down on the table move us to such

(Continued on inside back cover)

THIS PUBLICATION IS REPRODUCED BY AGREEMENT WITH THE COPYRIGHT OWNER. EXTENSIVE DUPLICATION OR RESALE WITHOUT PERMISSION IS PROHIBITED.